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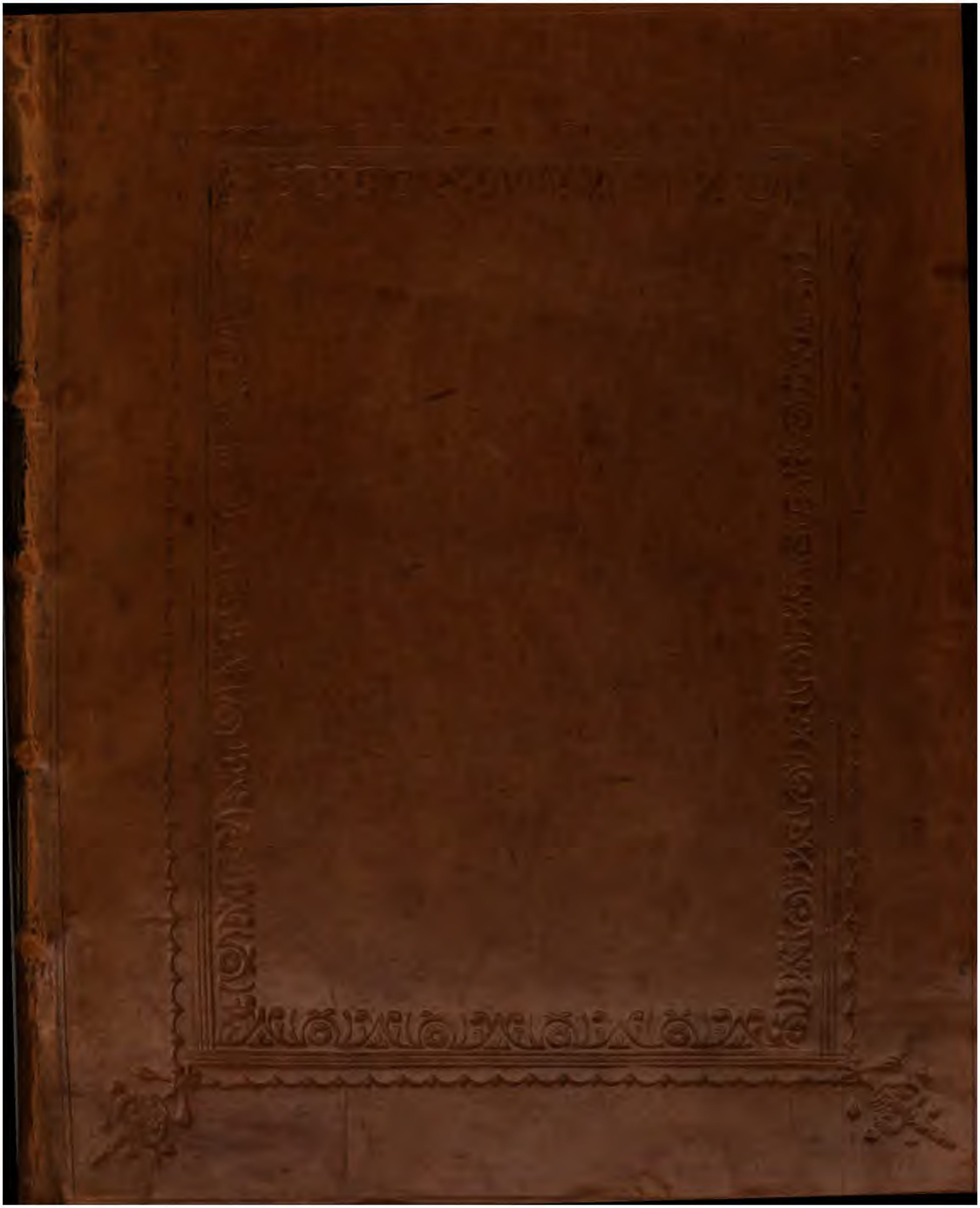

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PHILOSOPHICAL

TRANSACTIONS.

PART I.

PHILOSOPHICAL TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

VOL. LXXI. For the Year 1781.

PART I.

BODLEIAN · BIBLIOTHECA

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SOLD BY LOCKYER DAVIS, AND PETER ELMSLY,
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MDCCLXXXI.

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philoſophical Tranſactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations, which have been made in ſeveral former *Tranſactions*, that the printing of them was always, from time to time, the ſingle act of the reſpective Secretaries, till the Forty-ſeventh Volume: the Society, as a body, never intereſting themſelves any further in their publication, than by occaſionally recommending the revival of them to ſome of their Secretaries, when, from the particular circumſtances of their affairs, the *Tranſactions* had happened for any length of time to be intermitted. And this ſeems principally to have been done with a view to ſatisfy the Public, that their uſual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their firſt inſtitution by the Royal Charters, and which they have ever ſince ſteadily purſued.

But the Society being of late years greatly inlarged, and their communications more numerous, it was thought adviſeable, that a Committee of their members ſhould be appointed to reconſider the papers read before them, and ſelect out of them ſuch, as they ſhould judge moſt proper for publication in the future *Tranſactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and ſingularity of the ſubjects, or the advantageous manner of treating them; without pretending to anſwer for the certainty of the facts, or propriety of the reaſonings, contained in the ſeveral papers ſo publiſhed, which muſt ſtill reſt on the credit or judgment of their reſpective authors.

It is likewiſe neceſſary on this occaſion to remark, that it is an eſtabliſhed rule of the Society, to which they will always adhere, never to give their opinion, as a body, upon any ſubject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently propoſed from the chair, to be given to the authors of ſuch papers, as are read at their accuſtomed meetings, or to the perſons through whoſe hands they receive them, are to be conſidered in no other light than as a matter of civility, in return for the reſpect ſhewn to the Society by thoſe communications. The like alſo is to be ſaid with regard to the ſeveral projects, inventions, and curioſities of various kinds, which are often exhibited to the Society; the authors whereof, or thoſe who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the higheſt applauſe and approbation. And therefore it is hoped, that no regard will hereafter be paid to ſuch reports, and public notices; which in ſome inſtances have been too lightly credited, to the diſhonour of the Society.

A

SPEECH

DELIVERED TO

THE ROYAL SOCIETY,

ON WEDNESDAY NOVEMBER 30, 1780,

BEING THEIR ANNIVERSARY.

BY JOSEPH BANKS, ESQ.

PRESIDENT.

PRINTED BY ORDER OF THE COUNCIL.

THE Emotions of Gratitude inſpired by the very Place in which, by the Munificence of our Royal Patron, we are now for the firſt Time aſſembled, render it impoſſible for me to neglect the Opportunity which this Seaſon, when ye have been uſed to hear yourſelves addreſſed from the Chair, affords me, of offering my ſmall Tribute of Acknowledgement for a Benefit ſo eminently calculated to promote the Honour and Advancement of this Society.

Eſtabliſhed originally by the Munificence of a Royal Founder; foſtered and encouraged ſince that Time by every ſucceſſive Monarch who has ſwayed the Britiſh Sceptre, ye have ever proved yourſelves worthy the Favor of your Royal Protectors. A NEWTON, who pruned his infant Wing under your Auſpices, when his maturer Flights ſoared to Worlds unmeaſurably diſtant, ſtill thought a Place among you an honorable Diſtinction. A NEWTON's immortal Labors, a BOYLE, a FLAMSTEAD, a HALLEY, a RAY, and many others, of whom I truſt it is needleſs to remind you, have made ample Returns for the Patronage of former Monarchs.

But bountiful as the Encouragement ye have received from former Patrons has ever been, the Favors which Science has, through your Interceſſion, received from his preſent Majeſty (whom GOD long preſerve!) have eminently outdone their moſt extenſive Ideas of Liberality. Ample Funds, by Him provided, have enabled you to reward Men of extenſive Knowledge and Ability, for ſpending whole Years in the Service of Science; obſerving twice the Tranſit of the Planet Venus over the Diſk of the Sun. At your Requeſt, the Publick defrayed the Expence of convey-ing them to the moſt diſtant Parts of the Globe we inhabit, where the purpoſes of their Miſſion, ſo important to the Science of Aſtronomy, could beſt be fulfilled; while ye alone enjoy among your Fellow Academies the Reputation of having both ſent and rewarded them.

And more; thoſe very Donations were ſo liberally planned by that Attention to Science which has ever diſtinguiſhed His preſent Majeſty's Reign, and will for ever bear Teſtimony of his enlarged Mind, and Diſpoſition favorable to the Advancement of true Knowledge, that the Surplus alone enabled you, with his Royal Approbation, to inſtitute Experiments on the Attraction of Mountains, amidſt the barren and bleak Precipices of the Highlands of Scotland, which then, for the firſt Time, beheld Inſtruments of the niceſt Conſtruction tranſported to the Summits of their pathleſs Crags, and Men, uſed to other Habitations, voluntarily reſiding in temporary Huts, eager to expreſs a grateful Senſe of their Royal Patron's Liberality, by thus promoting to the utmoſt the Cauſe of Science, in which they were, under his Protection, embarked.

Gifts like theſe, unſolicited and unconditionally beſtowed, might have ſatisfied the Impulſes even of a Princely Munificence; but not ſo with our Royal Patron. Amply informed in every Branch of real Knowledge, He reſolved to beſtow a ſtill more diſtinguiſhed Mark of his Favor on Science which he loved, and in this his laſt beſt Gift has fulfilled his Royal Reſolution.

Such a Donation, ſo ſuited to our preſent proſperous and flouriſhing Condition under his Royal Patronage and Protection, is admirably calculated to increaſe the Reſpect, great as it is, which ye have ever received from the Learned of all Europe, placing you at once, in every Point of ſplendid Accommodation, as much above all Foreign Academies, as the Labors of your learned Predeceſſors had raiſed you above them in literary Reputation.

Let then Gratitude to a Sovereign, from whom ye have received ſuch conſpicuous Encouragement, engage you, by an Application to a Promotion of the Sciences ye ſeverally poſſeſs, to deſerve a Continuance of his Royal Favor; to meaſure your future Exertions by the Standard of his princely Liberality; and thus ſhew the World, that ye ſtill are, as ye always have been, worthy the Patronage of your King!

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PHILOSOPHICAL TRANSACTIONS.

I. *Natural Hiſtory and Deſcription of the* Tyger-cat *of the* Cape of Good Hope. *By* John Reinhold Forſter, *LL.D. F. R. and A. S.*

Read November 9. 1780.

FEW tribes of quadrupeds have in Africa more repreſentatives of their different ſpecies than that of the CAT. The genus of *Antelopes* may perhaps be excepted, ſince, to my knowledge, about twenty different *Ghazels* and *Antelopes* are to be met with in Africa; but no more than about eight or nine of the Cat tribe have hitherto been diſcovered on that continent. However, I know about twenty-one different ſpecies of this great claſs; and, I ſuppoſe, theſe by no means exhauſt this numerous tribe.

The greater and more numerous the different genera of animals are, the more difficult it muſt be to the natural hiſtorian properly to arrange the whole of ſuch an extenſive diviſion of animals, eſpecially if they are not equally well known. To form new genera, in order to diſpoſe and arrange them under, is a remedy which increaſes the evil, inſtead of curing it. The beſt method, therefore, which can be deviſed, is to make great diviſions in each genus, comprehending thoſe ſpecies which, on account of ſome common relation or character, have a greater affinity to one another. The genus of Cat, to which the animal belongs we are going to ſpeak of more at large, offers three very eaſy and natural ſub-diviſions. The *firſt* comprehends animals related to the Cat-tribe, with long hair or manes on their necks; *ſecondly*, ſuch as have remarkable long tails without any marks of a mane on their necks; *laſtly*, ſuch as have a bruſh of hair on the tips of their ears, and ſhorter tails than the ſecond ſub-diviſion. The firſt might be called in Latin *Feles jubatæ*; the ſecond ſub-diviſion ſhould be named *Ælures*; and the third and laſt, *Lynces*. To the firſt ſub-diviſion the *Lion* and the *hunting Leopard* or Indian *Chittah*, belong. The ſecond ſub-diviſion conſiſts of the *Tyger*, the *Panther*, the *Leopard*, the *Ounce*, the *Puma*, the *Jaguar-ete*, the *Jaguara*, the *Ocelot*, the *Gingy* of Congo, the *Marakaya*, the Tyger-cat of the Cape or the *'Nſuſſi* of *Congo*, the *Tibetan Tyger-cat* which I ſaw at Peterſburg, the common *Buſh-cat of the Cape*; and, laſtly, the *wild Cat*, and its domeſtic varieties. To the third diviſion belong the *Lynx*, the *Caracal*, the *Serval*, the *Bay Lynx*, and the *Ghaus* of Profeſſor GULDENSTEDT.

Since it is quite foreign to my purpoſe to ſpeak of thoſe ſpecies which are known already to the naturaliſt, I confine myſelf to that ſpecies only which hitherto has been imperfectly known to naturaliſts.

The firſt notice we had of the Cape Cat is, in my opinion, to be met with in LABAT's *Relation Hiſtorique de l'Ethiopie occidentale*, tom. I. p. 177. taken as is ſuppoſed from Father CARAZZI. LABAT mentions there the '*Nſuſſi*, a kind of wild Cat of the ſize of a Dog, with a coat as much ſtriped and varied as that of a *Tyger*. Its appearance beſpeaks cruelty, and its eyes fierceneſs; but it is cowardly, and gets its prey only by cunning and inſidious arts. All theſe characters are perfectly applicable to the Cape Cat, and it ſeems the animal is found in all parts of Africa, from Congo to the Cape of Good Hope, in an extent of country of about eleven degrees of latitude. KOLBE, in his *Preſent State of the Cape Good Hope*, vol. II. p. 127. (of the Engliſh edition) ſpeaks of a *Tyger Buſh-cat*, which he deſcribes as the largeſt of all the wild Cats of the Cape-countries, and is ſpotted ſomething like a Tyger. A ſkin of this animal was ſeen by Mr. PENNANT in a furrier's ſhop in London, who thought it came from the Cape of Good Hope; from this ſkin Mr. PENNANT gave the firſt deſcription which could be of any utility to a natural hiſtorian *. All the other authors mention this animal in a vague manner. When I and my ſon touched the ſecond time at the Cape of Good Hope in the year 1775, an animal of this ſpecies was offered me to purchaſe; but I refuſed buying it becauſe it had a broken leg, which made me apprehenſive of loſing it by death during the paſſage from the Cape to London. It was very gentle and tame. It was brought in a baſket to my apartment, where I kept it above four and twenty hours, which gave me the opportunity of deſcribing it, and of obſerving its manners and œconomy; as it did to my ſon that of making a very accurate drawing of it.

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* PENNANT's Synopſis of Quadrupeds, p. 181. firſt edit.

After a moſt minute examination, I found its manners and œconomy perfectly analogous to thoſe of our domeſtic Cats. It ate freſh raw meat, and was very much attached to its feeders and benefactors: though it had broke the fore-leg by accident, it nevertheleſs was very eaſy. After it had been ſeveral times fed by me, it ſoon followed me like a tame favourite Cat. It liked to be ſtroked and careſſed; it rubbed its head and back always againſt the perſon's cloaths who fed it, and deſired to be made much of. It purred as our domeſtic Cats do when they are pleaſed. It had been taken when quite young in the woods, and was not above eight or nine months old; I can, however, poſitively aver, having ſeen many ſkins of full-grown Tyger-cats *, that it had already very nearly, if not quite, attained its full growth. I was told, that the Tyger-cats live in mountainous and woody tracts, and that in their wild ſtate they are very great deſtroyers of Hares, Rabbits, Yerbuas, young Antelopes, Lambkins, and of all the feathered tribe.

DESCRIPTIO FELIS CAPENSIS.

Felis cauda ſub-elongata, annulata; corpore fulvo, ſupra maculis virgatis infra orbicularibus, auriculis nigris, macula lunata alba.

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* Theſe ſkins, with ſeveral others of rare and non-deſcript animals, I bought at a very conſiderable expence, and depoſited in the Britiſh Muſeum, that valuable national repoſitory of artificial and natural curioſities.

'Nſuſſi. LABAT Ethiopic. occident. tom. I. p. 177.

Tyger Boſch-katten. KOLBE Cape of Good Hope, vol. II. p. 127. (Engl. edit.)

Cape-cat. PENNANT Syn. Quadrup. p. 181. (1ſt edit.)

Corpus magnitudine *Felis Cati ſylveſtris* vel paulo majus. In genere ſupra colore pallide fulvo, ſubtus e cinereo albo, maculis atris.

(*Pili* apice pallide fulvi, baſi albi.)

Caput. *Roſtro* magis acuto, quam F. Cati, albo; in labio ſuperiore prope angulum rictus macula orbiculata, nigra. *Nares* nudæ, atræ. *Myſtaces* plurium ordinum in labio ſuperiore et ſupra oculos validæ, albæ et fulvæ. *Oculi* figura fere trianguli ſphærici, latere anteriore perpendiculari; *Irides* flavæ. *Pupilla* orbicularis, diurna (nec ut in F. Cato perpendiculari rima lemniſcata). *Lingua* retroſum aculeata. *Dentes* acuti ut in cogeneribus.

Linea albida utrinque naſo parallela, ad interiora oculorum latera. *Linea nigra* paullulum convergens a cantho anteriore oculorum deſcendit in naſum; *aliæ duæ nigræ* ſupra oculos infra convergentes, inque frontem aſcendentes; præterea in capite *puncta et lineolæ nigræ* plures ſparſæ.

Auriculæ amplæ, longitudine fere capitis, ovatæ, ſuberectæ, *intus* villoſæ, ochroleucæ; *extus* nigræ, macula lunata, tranſverſa alba. *Margo exterior* ſacculo membranaceo nudo, lobato.

Corpus ovatum, elegans. *Lineæ atræ* longitudinales quatuor in cervice inter aurium baſes orſæ, in *Dorſo* interruptæ; *Superiora laterum* obtinent maculæ oblongæ, lineares, obliquæ. *Inferiora laterum* maculis rotundis ſparſis. *Abdomen* e cinereo-albo, maculis rotundis parvis, ſparſis, nigris.

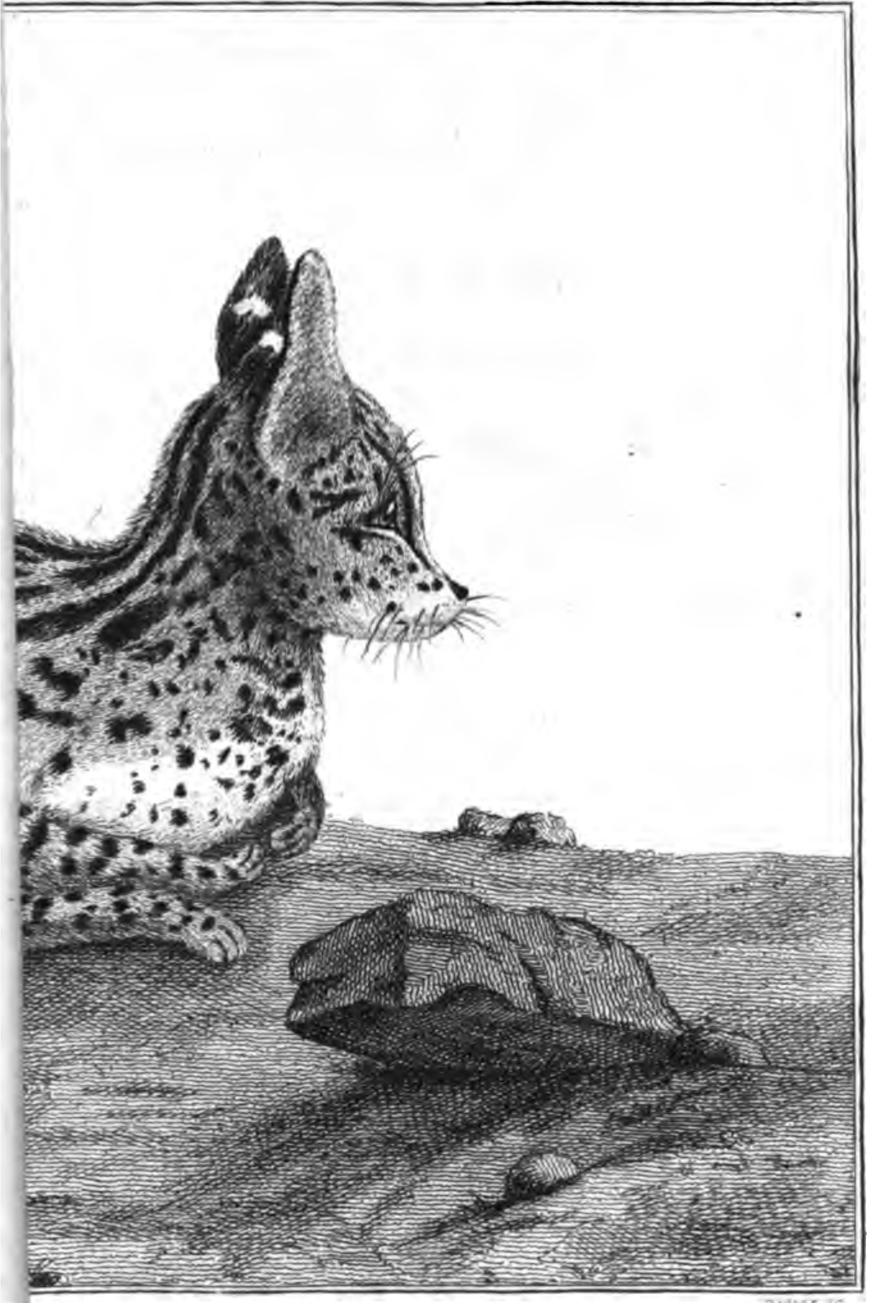
Pedes omnes ſuperne ſubfaſciati, extremitatibus punctis numeroſis, nigris conſperſi. *Digiti* quinque felini. *Ungues* modici, retractiles, nigri.

Cauda attingit baſin tarſi, annulis circiter octo vel decem nigris cincta.

M E N S U R Æ.

Ab apice roſtri ad baſin caudæ	18 unciæ ped. Angl.
Cauda	8
Caput longum	4
Auriculæ margine exteriore	3
Pedes anteriores a cubito	7
Pedes poſtici (tarſi ſcilicet)	4½

Philos. Trans. Vol. LXXI. Tab. 1. *p. 6.*



BASIRE sc.

II. *Experiments and Obſervations on the ſpecific Gravities and attractive Powers of various ſaline Subſtances.* *By* Richard Kirwan, *Eſq. F. R. S.*

Read November 16, 1780.

THE doctrine of chymical affinities hath of late received great improvements from the labours of the very excellent Mr. BERGMAN of Upſal, and the ſtill later reſearches of Mr. WENTZEL; but the order of theſe attractions has hitherto been the only point attended to by theſe philoſophers, as well as by moſt preceding chymiſts: for I know of none, except Mr. MORVEAU of Dijon, who has thought of aſcertaining the various degrees of force of chemical attraction, by which one body acts on various other bodies, or even on the ſame body in various circumſtances. He has, however, ſo ably ſhewn the advantages ariſing from ſuch an inquiry, that I have made it the object of my attention, and beſtowed much pains on it for ſome time paſt; and have been thereby enabled to determine pretty exactly the proportion of the ingredients of many neutral ſalts, and the ſpecific gravity of the mineral acids in their pureſt ſtate, and free from all water. The principles on which theſe determinations are founded are the following.

1ſt. That the ſpecific gravity of bodies is as their weight, divided by the weight of an equal bulk of rain or diſtilled water, this being at preſent the ſtandard with which every other body is compared.

2dly. That if bodies, ſpecifically heavier than water, be weighed in air and in water, they loſe in water part of the weight which they were found to have in air; and that the weight ſo loſt is juſt the ſame as that of an equal bulk of water, and conſequently that their ſpecific gravity is equal to their weight in air, or *abſolute weight*, divided by their loſs of weight in water.

3dly. That if a ſolid, ſpecifically heavier than a liquid, be weighed firſt in air, and then in that liquid, the weight it loſes is equal to the weight of an equal volume of that liquid; and conſequently if ſuch ſolid be weighed firſt in air, then in water, and afterwards in any other liquid, the ſpecific gravity of this liquid will be as the weight loſt in it by ſuch ſolid, divided by the loſs of weight of the ſame ſolid in water. This method of finding the ſpecific gravity of liquids I have found much more exact than that by the areometer, or the compariſon of weights of equal meaſures of ſuch liquids and water, both of which are ſubject to ſeveral inaccuracies.

4thly. That where the ſpecific gravity of bodies is already known, the weight of an equal bulk of water may alſo be found, it being as the quotient of their abſolute weight divided by their ſpecific gravity. This I ſhall call their *loſs of weight* in water.

Hence, where the ſpecific gravity and abſolute weight of the ingredients of any compound are known, the ſpecific gravity of ſuch compound may eaſily be calculated as it ought to be intermediate betwixt that of the lighter and that of the heavier, according to their ſeveral proportions: this I call the *mathematical* ſpecific gravity. But, in fact, the ſpecific gravity of compounds, found by actual experiment, ſeldom agrees with that found by calculation, but is often greater without any diminution of the lighter ingredient. This increaſe of denſity muſt then ariſe from a cloſer union of the component parts to each other than either had ſeparately with its own integrant parts; and this more intimate union muſt proceed from the attraction or affinity of theſe parts to each other: I therefore imagined this attraction might be eſtimated by the increaſe of denſity or ſpecific gravity and was proportionable to it, but was ſoon undeceived.

I muſt alſo premiſe, that the abſolute weights of many ſorts of air have been accurately determined by Mr. FONTANA, at whoſe experiments I was preſent, the thermometer being at 55°, and the barometer at 29½ inches, or nearly ſo. Their weights were as follows:

Cubic inch of common air -	0,385
Fixed air - - -	0,570
Marine air - - -	0,654
Nitrous air - - -	0,399
Vitriolic air - - -	0,778
Alkaline air - - -	0,2
Inflammable air - -	0,03

OF SPIRIT OF SALT.

From the time I firſt read in Dr. PRIESTLEY's Experiments on Air (that inexhauſtible ſource of future diſcoveries) of the exhibition of marine acid in the form of air, free from water; and that this air, reunited with water, formed an acid liquor in all reſpects the ſame as common ſpirit of ſalt; I conceived the poſſibility of diſcovering the exact quantity of acid in ſpirit of ſalt of any given ſpecific gravity, and by means of this the exact proportion of acid in all other acid liquors; for if a given quantity of pure fixed alkali were ſaturated, firſt by a certain quantity of ſpirit of ſalt, and then by determined quantities of the other acids, I concluded, that each of theſe quantities of acid liquor muſt contain the ſame quantity of acid, and this being known, the remainder being the aqueous part, this alſo muſt be known; but this concluſion intirely reſted on the ſuppoſition that the ſame quantity of all the acids was requiſite for the ſaturation of a given quantity of fixed alkali; for if ſuch given quantity of fixed alkali might be ſaturated by a ſmaller quantity of one acid than of another, the concluſion fell to the ground. This point might, indeed, be in ſome meaſure determined by weighing the neutral ſalts, formed by theſe acids, when thoroughly dry; but ſtill a ſource of inaccuracy remained: for if they were expoſed to a conſiderable heat, part of the acid would neceſſarily be expelled, and more of one acid than of another, and if the heat were not conſiderable, much of the water of cryſtallization would remain; ſo that if the weights were found to be equal, this equality could not be aſcribed to equal quantities of acid, but might perhaps ariſe from a ſmaller proportion of acid in one of them, and a larger proportion of water, and in another from a larger proportion of acid and a ſmaller proportion of water; and if the weights were unequal, no certain concluſion could be drawn. To obviate this difficulty I uſed the following expedient. 1ſt. I ſuppoſed the quantities of nitrous and vitriolic acids, neceſſary to ſaturate a given quantity of fixed alkali, exactly the ſame as that of marine acid whoſe quantity I determined; and to prove the truth of this ſuppoſition, I obſerved the ſpecific gravity of the ſpirit of nitre and oil of vitriol I made uſe of, and in which I ſuppoſed, from the trial with alkalies, a certain proportion of acid and water; I then added to theſe more acid and water, and calculated what their ſpecific gravities ſhould be upon the above ſuppoſition, and finding the reſult to tally with the ſuppoſition, I concluded the latter to be exact.

The experiments made on the marine acid were as follows.

I took two bottles, which I filled nearly to the top with diſtilled water, of which they contained in all 1399,9 gr. and introduced them ſucceſſively into two cylinders filled with marine air, which I had obtained from common ſalt by means of dilute oil of vitriol and heat, in a mercurial apparatus; and this proceſs I renewed until the water had imbibed, in eighteen days, about 794 cubic inches of the marine air. The thermometer did not riſe all this time above 55°, nor ſink, unleſs perhaps at night, under 50°, and the barometer was between 29 and 30 inches. This water, or rather ſpirit of ſalt, I then found to weigh 1920 gr. that is 520,1 more than before. The quantity of marine air abſorbed amounted then to 520,1 gr. I then examined the ſpecific gravity of this ſpirit of ſalt, and found it to be 1,225. Its loſs of weight in water (that is, the weight of an equal bulk of water) ſhould then be 1567,346 gr. nearly; but it contained only, as we have ſeen, 1399,9 gr. of water: therefore ſubtracting this from 1567,346, the remainder (that is, 167,446) muſt be the loſs of 520,1 gr. of marine acid; and conſequently the ſpecific gravity of the pure marine acid, in ſuch a condenſed ſtate as it is in when united to water, muſt be $\frac{520,1}{167,446} = 3,100$. But ſtill it might be ſuſpected, that the denſity of this ſpirit did not intirely proceed from the mere denſity of the marine acid, but in part alſo from the attraction of this acid to water, and though the length of time requiſite to make water imbibe this quantity of acid made me judge that the attraction was not very conſiderable, yet the following experiment was more ſatisfactory.

I expoſed 1440 gr. of this ſpirit to marine air for five days, the thermometer being at 50° or under; it then weighed 1562 gr. and conſequently imbibed 122 gr. of marine air; its ſpecific gravity was then 1,253, which agrees exactly with what it ſhould be by calculation.

N. B. I have not repeated the whole of theſe experiments, as they were very tedious; but I began them over again ſeveral times before I could aſcertain with any preciſion the quantity of marine air abſorbed, as, when the whole of a cylinder full of air was abſorbed, it was difficult to ſtop the bottles ſo as to prevent any mercury from falling in; and I was obliged every night to fill the cylinders with air, leſt if there remained but a ſmall quantity it might be imbibed before morning, and the mercury fall into the bottles. I alſo made ſome allowance for the common air which I could not avoid letting into the cylinder with the marine air, as will be very apparent to whoever repeats the experiment.

Being now ſatisfied I had diſcovered the proportion of acid and water in ſpirit of ſalt, I was impatient to find it in other acids alſo; and for that purpoſe I took 180 gr. of very ſtrong oil of tartar *per deliquium*, but of whoſe ſpecific gravity I can find no note, and found it to be ſaturated by 180 gr. of ſpirit of ſalt, whoſe ſpecific gravity was 1,225. Now, by calculation it appears, that 180 gr. of this ſpirit contains 48,7 gr. of acid and 131,3 of water, and hence I drew up the following table.

Marine

Marine acid.	Water.	Specific Gravity.
Parts.	Parts.	
48,7	50	1,497
	60	1,431
	70	1,381
	80	1,341
	90	1,308
	100	1,282
	110	1,259
	120	1,246
	130	1,223
	140	1,209
	150	1,196
	160	1,185
	170	1,175
	180	1,166
	190	1,158
	200	1,151
	210	1,144
	220	1,138
	230	1,132
	240	1,127
	250	1,122
	260	1,118
	270	1,114
	280	1,110
	290	1,106
	300	1,103
	310	1,100
	320	1,097
	330	1,091
	340	1,089
	350	1,086
	360	1,084
	370	1,082
	380	1,080
	390	1,078
	400	1,076
	410	1,074

The ſpecific gravity of the ſtrongeſt ſpirit of ſalt, made in the uſual way, is, according to Mr. BAUME, 1,187, and according to Mr. BERGMAN, 1,190; but we read in the Paris Memoirs for the year 1700, p. 191. that Mr. HOMBERG paſſed a ſpirit whoſe ſpecific gravity was 1,300; and that made by Dr. PRIESTLEY (ſee vol. III. p. 275.) muſt have been about 1,500.

Hence we ſee, that ſpirit of ſalt, whoſe ſpecific gravity is 1,261 or leſs, has little or no attraction with water, and therefore attracts none from air, and on that account does not heat a thermometer whoſe ball is dipped in it as ſpirit of vitriol and ſpirit of nitre do, as has lately been obſerved by the Friendly Society of Berlin.

This table is not exactly accurate, as I had not in this firſt experiment found the point of ſaturation as nicely as was requiſite. However, I have not corrected it, as the error is but ſmall, and the proportion may at any time be found by calculation; at leaſt when the ſpecific gravity of this ſpirit does not exceed 1,253. Whether the mathematical ſpecific gravity and that by obſervation differ in the higher degrees of ſpecific gravity, I have not examined; but the table is formed on the ſuppoſition that they do not.

Common ſpirit of ſalt is always adulterated with vitriolic acid, and therefore not fit for theſe trials.

Intending to determine by this experiment the proportion of acid, water, and fixed alkali in digeſtive ſalt, as it is called, I took 100 gr, of a ſolution of a tolerably pure vegetable alkali that had been three times calcined to whiteneſs, the ſpecific gravity of which ſolution was 1,097. I alſo diluted the ſpirit of ſalt with different portions of water; the ſpecific gravity of one ſort was 1,115, and of another 1,098.

I then found that the above quantity of the ſolution of a vegetable alkali required for its ſaturation 27 gr. of that ſpirit of ſalt whoſe ſpecific gravity was 1,098, and 23,35 gr. of that ſpirit of ſalt whoſe ſpecific gravity was 1,115. Now, 27 gr. of ſpirit of ſalt, whoſe ſpecific gravity is 1,098, contain 3,55 gr. of marine acid, as appears by calculation. As the principle on which this calculation, by which the proportion of ſubſtances in alloy is found, may not be generally known, I ſhall here mention them in the words of Mr. COTES.

" The *data* requiſite are the ſpecific gravities of the mixture " and of the two ingredients. . . . . Then, as the difference " of the ſpecific gravities of the mixture and the lighter ingre- " dient is to the difference of the ſpecific gravities of the mix- " ture and the heavier ingredient, ſo is the magnitude of the " heavier to the magnitude of the lighter ingredient. Then, " as the magnitude of the heavier multiplied into its ſpecific " gravity is to the magnitude of the lighter multiplied into its " ſpecific gravity, ſo is the weight of the heavier to the weight " of the lighter . . . . Then, as the ſum of theſe weights " is to the given weight of either ingredient, ſo is the weight " given to the weight of the ingredient ſought."

Thus, in this caſe, 1,098 - 1,000 = ,098 is the magnitude of the heavier ingredient, *viz.* the marine acid; and, 098 × 3,100 = 0,3038 the weight of the marine acid; and, on the other hand, 3,100 - 1,098 = 2,002 the magnitude of the water, and 2,002 × 1,000 = 2,002 its weight; the ſum of theſe weights is 2,3058: then, if 2,3058 parts of ſpirit of ſalt contain 0,3038 parts acid, 27 gr. of this ſpirit of ſalt will contain 3,55 acid.

In the ſame manner it will be found, that 23,35 gr. of ſpirit of ſalt, whoſe ſpecific gravity was 1,115, contained 3,55 gr. acid.

The point of ſaturation was pretty accurately found by putting the glaſs cylinder which contained the alkaline ſolution on the ſcale of a very ſenſible balance, and at the ſame time weighing the acid liquor in another pair of ſcales, when the loſs of weight indicated the eſcape of nearly equal quantities of the fixed air contained in the ſolution; then the acid was gradually added, by dipping a glaſs rod into it, to the top of which a ſmall drop of acid adhered: with this the ſolution was ſtirred, and very ſmall drops taken up and laid on bits of paper ſtained blue with radiſh juice. As ſoon as the paper was in the leaſt reddened, the operation was completed ſo that there was always a very ſmall exceſs of acid, for which half a grain was conſtantly allowed; but no allowance was made for the fixed air, which always remains in the ſolution; but as, on this account, only a ſmall quantity of the alkaline ſolution was uſed, this proportion of fixed air muſt have been inconſiderable. If an ounce of the ſolution had been employed, this inappreciable portion of fixed air would be ſufficient to cauſe a ſenſible error: for I judged of the quantity of fixed air loſt by the difference betwixt the weight added to the 100 gr. and the actual weight of the compound. When this difference amounted to 2,2 gr. then I judged the whole of the fixed air expelled, and found it to be ſo, as 100 gr. of this alkaline ſolution, being evaporated to dryneſs in a heat of 300°, left a reſiduum which amounted to 10½ gr.; which 10½ gr. contained 2,2 gr. of fixed air, as will hereafter be ſeen.

Hence 8,3 gr. of pure vegetable fixed alkali, free from fixed air and water, or 10,5 of mild fixed alkali, were ſaturated by 3,55 gr. of pure marine acid, and conſequently the reſulting neutral ſalt ſhould, if it contained no water, weigh 11,85 gr.; but the ſalts reſulting from this union (the ſolution being evaporated to perfect dryneſs in a heat of 160° kept up for four hours) weighed at a medium 12,66 gr. Of this weight 11,85 gr. were acid and alkali; therefore the remainder, *viz.* 0,81 of gr. were water; therefore 100 gr. of perfectly dry, digeſtive ſalt contain 28 gr. acid, 6,55 water, 65,4 of fixed alkali.

I was then curious to compare my experiments with thoſe made by others, but could not find any made with ſufficient preciſion but thoſe of Mr. HOMBERG in the Paris Memoirs for 1699. However, as to ſpirit of ſalt I did not think proper to compare them, as he mentions that his could diſſolve gold, and therefore was probably impure.

OF SPIRIT OF NITRE.

The common reddiſh brown or greeniſh ſpirit of nitre containing, beſides acid and water, a certain portion of phlogiſton, and being alſo mixed with ſome portion of the acid of ſea ſalt, I judged unfit for theſe trials, and therefore uſed only the dephlogiſticated ſort, which is quite colourleſs, and reſembles pure water in its appearance. This pure acid cannot be made to exiſt in the form of air, as Dr. PRIESTLEY has ſhewn; for when it is deprived of water and phlogiſton, and furniſhed with a due proportion of elementary fire, it ceaſes to have the properties of an acid, and becomes dephlogiſticated air; I could not, therefore, determine its proportion in ſpirit of nitre as I had done that of the marine acid, but was obliged to uſe another method.

1ſt. To 1963,25 gr. of this ſpirit of nitre, whoſe ſpecific gravity was 1,419, I gradually added 179,5 gr. of diſtilled water, and when it cooled I found the ſpecific gravity of this mixture 1,389.

2dly. To 1984,5 gr. of this I again added 178,75 gr. of water; its ſpecific gravity was then 1,362.

I then took 100 gr. of a ſolution of fixed vegetable alkali, whoſe ſpecific gravity was 1,097, the ſame I had before uſed in the trials with ſpirit of ſalt, and found this quantity of alkali to be ſaturated by 11 gr. of the ſpirit of nitre, whoſe ſpecific gravity was 1,419; and by 12 gr. of the ſpirit, whoſe ſpecific gravity was 1,389; and by 13,08 of that, whoſe ſpecific gravity was 1,362. The quantities here mentioned were the medium of five experiments. I found it neceſſary to dilute the nitrous acid with a ſmall proportion of water, of which I kept an account. When I neglected this precaution, I found that part of the acid was phlogiſticated, and went off with the fixed air. Note alſo, that after each affuſion of acid ten minutes were allowed to the matters to unite, a precaution which I alſo found abſolutely neceſſary.

Hence (upon the ſuppoſition that a given quantity of fixed vegetable alkali is ſaturated by the ſame weight of both acids) we ſee that 11 gr. of ſpirit of nitre, whoſe ſpecific gravity is 1,419, contain the ſame quantity of acid as 27 gr. of ſpirit of ſalt, whoſe ſpecific gravity is 1,098, that is, 3,55 gr.; the remainder of 11 gr. is therefore mere water, *viz.* 7,45 gr.; conſequently, if the denſity of the acid and water had not been increaſed by their union, the ſpecific gravity of the pure and mere nitrous acid ſhould be 11,8729; for the ſpecific gravity of this acid ſhould be as its abſolute weight divided by its loſs of weight in water, and this loſs ſhould be as the total loſs of theſe 11 gr. minus the loſs of the aqueous part. Now the total loſs $= \frac{11}{1,419} = 7,749$, and the loſs of the aqueous part $= 7,45$, and conſequently the loſs of the acid part is $7,749 - 7,45 = 0,299$, and therefore the ſpecific gravity of the acid part, that is, of the pure nitrous acid, is $\frac{3,55}{0,296} = 11,8729$.

But it is well known, that the denſity of the nitrous acid, as well as that of the vitriolic, is increaſed by its union with water; and therefore the loſs above found is not the *whole* of its real loſs in its natural ſtate (if it could be ſo found), but partly the loſs that ariſes from the denſity that accrues to it from its union with water: for ſince its denſity is increaſed by this union, its loſs is leſs than it would be if the nitrous acid had only its own proper denſity, and conſequently the ſpecific gravity above found is greater than its real ſpecific gravity.

To determine, therefore, the real ſpecific gravity of this acid in its natural ſtate, the quantity of *accrued* denſity muſt be found, and ſubtracted from the ſpecific gravity of the ſpirit of nitre, whoſe true mathematical ſpecific gravity will then appear. I endeavoured to effect this by mixing different portions of ſpirit of nitre and water, remarking the diminution of their joint volume below the ſum of the ſpaces occupied by their ſeparate volumes; but could never attain a ſufficient degree of preciſion. The following method, though not exactly accurate, I found more ſatisfactory. 12 gr. of the ſpirit of nitre, whoſe ſpecific gravity by obſervation was 1,389, contained as I ſuppoſed from the former experiment 3,55 gr. of acid, and 8,45 of water; then if the ſpecific gravity of the pure nitrous acid were 11,872, the ſpecific gravity of this compound of acid and water ſhould be 1,371; for the loſs of 3,55 gr. acid ſhould be 0,299, and the loſs of the water 8,45; the ſum of the loſſes 8,749. $\frac{12}{8,749} = 1,371$; but, as I already ſaid, the ſpecific gravity by obſervation was 1,389, therefore the *accrued* denſity in this caſe was at leaſt ,018, the difference betwixt 1,389 and 1,371. I ſay *at leaſt*, for as the ſpecific gravity 11,872 was certainly too high, the loſs of 3,55 gr. acid was certainly too ſmall; and if it were greater, the mathematical ſpecific gravity 1,371 would have been ſtill lower. However, ,018 is certainly a near approximation to the degree of denſity that accrues to 3,55 gr. acid by their union to 7,45 gr. of water, and differs inconſiderably from the truth, as will appear by the ſequel: therefore ſubtracting this quantity from 1,419 we have nearly the mathematical ſpecific gravity of that proportion of acid and water, namely, 1,401. And ſince 11 gr. of this ſpirit of nitre contain 3,55 gr. acid and 7,45 of water, its loſs of weight ſhould be $\frac{11}{1,401} = 7,855$, and ſubtracting the loſs of the aqueous part from this, the remainder 0,405 is the loſs of the 3,55 gr. acid, and conſequently the true ſpecific gravity of the pure and mere nitrous acid is $\frac{3,55}{0,405} = 8,7654$: this being ſettled, the mathematical ſpecific gravity and true increaſe of denſity of the above mixtures will be found. Thus the mathematical ſpecific gravity of 12 gr. of that ſpirit of nitre, whoſe ſpecific gravity by obſervation was 1,389, muſt be 1,355, ſuppoſing it to contain 3,55 gr. acid and 8,45 of water; for the loſs of 3,55 gr. acid is $\frac{3{,}55}{8{,}763}=0{,}405$, and the loſs of water 8,45; the ſum of theſe loſſes is 8,855. Then, $\frac{12}{8{,}855}=1{,}355$, and conſequently the accrued denſity is $1{,}389-1{,}355={,}034$. In the ſame manner it will be found, that the mathematical ſpecific gravity of 13,08 gr. of that ſpirit of nitre, whoſe ſpecific gravity by obſervation was 1,362, muſt be 1,315, and conſequently its accrued denſity ,047.

But the whole ſtill reſts upon the ſuppoſition that each of theſe portions of ſpirit of nitre contain 3,55 gr. of acid. To verify this ſuppoſition, I could think of no better method than that of examining the mathematical ſpecific gravities of the firſt mixture I had made of ſpirit of nitre and water in large quantities; for if the mathematical ſpecific gravities of theſe agreed exactly with thoſe of the quantities I had ſuppoſed in ſmaller portions of each, I could not but conclude, that the ſuppoſitions of ſuch proportions of acid and water, as I had determined in each, was juſt; and that this was the caſe will appear by the following calculations.

1ſt. When to 1963,25 gr. of ſpirit of nitre, whoſe ſpecific gravity was 1,419, 179,5 gr. of water were added, the quantity of acid upon the above ſuppoſition ſhould be 634,53 gr.; for ∷ 11 . 3,55 :: 1963,25 . 634,53 . the quantity of water in thoſe 1963,25 gr. of ſpirit of nitre ſhould then be 1328,72, and after adding 179,5 gr. of water, the whole quantity of acid and water ſhould be 2142,75, the loſs of acid was $\frac{634{\cdot}53}{8{,}7654}=$ 71,24, and the ſum of the loſſes 1580,46: then the mathematical ſpecific gravity ſhould be $\frac{2142,75}{1580,46} = 1,355$, which is exactly the ſame as that which was found in 12 gr. of this ſpirit of nitre, on the ſuppoſition that they contained 3,55 gr. acid.

Again: when to 1984,5 gr. of this mixture I added 178,75 gr. of water, the whole quantity of diluted ſpirit of nitre was 2163,25 gr. and the quantity of acid in 1984,5 gr. was 587,081 gr. for ∺ 12 . 3,55 :: 1984,5 . 587,081; the loſs of this quantity of acid is 66,96 gr. and the ſum of the loſſes of acid and water is 1643,129 gr.; and conſequently the mathematical ſpecific gravity ſhould be $\frac{2163,75}{1643,125} = 1,315$, which is the ſame as that determined in 13,08 gr. of the ſame mixture.

By continuing theſe mixtures until I found the mathematical ſpecific gravity and that by obſervation nearly to coincide, I was enabled to draw up the following table, in which if any errors be found, I hope they will be excuſed, from the impoſſibility of avoiding them where the weights muſt be found with ſuch extreme preciſion: the two firſt ſeries were only found by analogy.

Spirit

Spirit of nitre.	Acid.	Water.	Accrued denſity.	Mathematical ſpecific gravity.	Specific gravity by obſervation	Attract. of the acid to water.	Attract. of water to the acid.
Grs.	Grs.	Grs.					
9	– –	5,45	,000	1,537	1,537	– –	– –
10	– –	6,45	,009	1,458	1,467	,009	,054
11	– –	7.45	,018	1,401	1,419	,018	,045
12	– –	8,45	,034	1,355	1,389	,027	,036
13,08	– –	9,53	,047	1,315	1,362	,036	,027
14,15	– –	10,6	,051	1,286	1,337	,045	,018
15.23	– –	11,68	,054	1,260	1,314	,054	,009
16,305	– –	12,755	,054	1,238	1,292	,054	,009
17,38	– –	13,83	,051	1,220	1,271		
18,445	– –	14,9	,047	1,205	1,252		
19,53	– –	15,98	,044	1,191	1,235		
20,605	– –	17,055	,042	1,180	1,222		
21,68	– –	18,13	,040	1,177	1,217		
22,755	– –	19,205	,038	1,160	1,198		
23,83	– –	20,28	,036	1,152	1,188		
24,905	– –	21,45	,033	1,144	1,177		
26,17	– –	22,62	,030	1,132	1,162		
27,34	3,55	23,79	,027	1,130	1,157		
28,51	– –	24,96	,026	1,124	1,150		
29,68	– –	26,13	,024	1,114	1,138		
30,85	– –	27,30	,022	1,113	1,135		
32,02	– –	28,47	,020	1,109	1,129		
33,09	– –	29,54	,018	1,102	1,120		
34,26	– –	30,71	,016	1,101	1,117		
35,43	– –	31,88	,014	1,097	1,111		
36,60	– –	33,05	,012	1,094	1,106		
37,77	– –	34,22	,010	1,090	1,100		
38,94	– –	35,39	,008	1,088	1,096		
40,11	– –	36,56	,006	1,085	1,091		
41,28	– –	37,73	,004	1,082	1,086		
42,45	– –	38,90	,002	1,080	1.082		

The intermediate ſpecific gravities may be found by taking an arithmetical mean between the ſpecific gravities by obſervation betwixt which that ſought lies, and noting how much it exceeds or falls ſhort of ſuch arithmetical mean; and then taking alſo an arithmetical mean betwixt the mathematical ſpecific gravities betwixt which that ſought for muſt lie, and a proportionate exceſs or defect.

I have added a column of attraction of the nitrous acid to water as far as it keeps pace with the increaſe of denſity, but no farther, as I am unacquainted with the law of its further increaſe.

The ſpecific gravity of the ſtrongeſt ſpirit of nitre yet made is, according to Mr. BAUME, 1,500; and according to Mr. BERGMAN, 1,586.

I next proceeded to examine the proportion of acid, water, and fixed alkali in nitre, in the ſame manner as I had before done that in digeſtive ſalt, and found that 100 gr. of perfectly dry nitre contain 28,48 gr. acid, 5,2 of water, and 66,32 of fixed alkali.

I ſhall now compare the reſult of theſe experiments with thoſe of Mr. HOMBERG.

The ſpecific gravity of the ſpirit of nitre which Mr. HOMBERG made uſe of was 1,349; and of this, he ſays, 1 oz. 2 dr. and 36 gr. that is, 621 Troy, are requiſite to ſaturate 1 French (oz. 472,5 Troy) of dry ſalt of tartar; according to my computation 613 gr. are ſufficient; for this ſpecific gravity lies between the tabular ſpecific gravities by obſervation 1,362 and 1,337, and is nearly an arithmetical mean between them. The correſponding mathematical ſpecific gravity lies betwixt the tabular quantities 1,315 and 1,286, and is nearly 1,300. Now, the proportion of acid and water in this is, 2,629 of acid, and 7,465 of water; for $\begin{array}{r} 8{,}765 \\ -\ 1{,}300 \\ \hline =7{,}465 \end{array}$ water and $\begin{array}{r} 8{,}765\times .300 \\ \hline =\ 2{,}629 \end{array}$ of acid; and the ſum of both is 10,044. Now, ſince 10,5 gr. mild vegetable fixed alkali require 3,55 gr. of acid for their ſaturation, 472,5 will require 159,7; therefore, if 10,044 gr. of nitre contain 2,629 gr. acid, the quantity of this ſpirit of nitre requiſite to give 159,7 will be 613,2 nearly, and hence the difference betwixt us is only about 8 gr.

2dly. Mr. HOMBERG ſays, he found his ſalt, when evaporated to dryneſs, to weigh 186 gr. more than before; whereas, by my experiment, it ſhould weigh but 92,8 gr. more than at firſt. I ſhall mention the cauſe of this difference in treating of tartar vitriolate, for it cannot be intirely attributed to the difference of evaporation.

3dly. Mr. HOMBERG infers, that 1 oz. (that is, 472,5 Troy gr.) of this ſpirit of nitre contains 141 gr. Troy of real acid: by my computation it contains but 123,08 gr. of real acid. This difference evidently proceeds from his neglecting the quantity of water that certainly enters into the compoſition of nitre; for he proceeds on this analogy, 621 . 186,6 :: 472,5 . 141.

The proportion of fixed alkali I have aſſigned to nitre is fully confirmed by a very curious experiment of Mr. FONTANA's, inſerted in ROZIER's Journal for November 1778. This ingenious philoſopher decompoſed 2 oz. of nitre by diſtilling it in a ſtrong heat for eighteen hours. After the diſtillation there remained in the retort a ſubſtance purely alkaline, amounting to 10 French dr. and 12 gr. Now 2 French oz. = 944 gr. Troy, and the alkaline matter amounts to 607 gr. Troy; and, according to my computation, 944 gr. of nitre ſhould contain 625 of alkali. So ſmall a difference may fairly be attributed to the loſs in transferring from one veſſel to another, weighing, filtering, evaporating, &c.

Mr. LAVOISIER, in the Paris Memoirs for the year 1776, has given us, after Dr. PRIESTLEY, the analyſis of the nitrous acid.

In 2 oz. French meaſure (=945 gr. Troy) of ſpirit of nitre, whoſe ſpecific gravity was 1,3160, he diſſolved 2 oz. and 1 dr. of mercury; the quantity of air obtained during the ſolution was 190 cubic inches French (=202,55 Engliſh). This air was all nitrous. There remained a white mercurial ſalt, which, being diſtilled, afforded 12 cubic inches (=12,785 Engliſh) of air mixed with red vapours, and which differed little from common air. There afterwards aroſe 224 cubic inches (=238,56 Engliſh) of dephlogiſticated air, during the production of which, the mercury was almoſt revivified, there remaining but a few grains of a yellow ſublimate. The 12 inches of air mixed with red vapours aroſe, he ſays, from a mixture of 36 cubic inches of nitrous air (=38,34 Engliſh) and 14 of dephlogiſticated air (14,91 Engliſh); and as the mercury was almoſt wholly revived, he concludes, theſe airs aroſe from the nitrous acid, and formed *it*; and hence infers, that 16 oz. of this ſpirit of nitre (=7560 gr. Troy) contained 13 oz. 7 dr. $36\frac{3}{5}$ gr. (that is, 6589 gr. Troy) of water, and conſequently only 971 gr. Troy of real acid, and therefore 2 oz. of this ſpirit of nitre contained but 120 gr. Troy of real acid: but, by my calculation, 2 oz. of this ſpirit of nitre contained 213 gr. acid; for its mathematical ſpecific gravity is 1,265. The ſame weight of acid will alſo be found in it by computing the weight of the volumes of the different airs he himſelf found it conſiſt of, or at leaſt to afford by its decompoſition; for 202,55 cubic inches of nitrous air weigh, by Mr. FONTANA's experiment, 80,8174 gr. Troy, and 238,56 inches of dephlogiſticated air weigh 100,1952 gr. Troy, and adding to theſe the weight of 38,34 inches of nitrous air, and of 14,91 of dephlogiſticated air, which made the 12 cubic inches of air mixed with red vapours, we ſhall find the whole weight of theſe airs to be 202,181 gr.;

the few grains wanting of 213 gr. may be accounted for from the abſorption of the water in which he received the airs, and by allowing for that ſtill remaining in the yellow ſublimate.

OF OIL OF VITRIOL.

The oil of vitriol I made uſe of was not perfectly dephlogiſticated; but though pale yet a little inclined to red. It contained ſome whitiſh matter, as I perceived by its growing milky on the affuſion of pure diſtilled water. How far this may alter the reſult of the following experiments I have not tried; but believe it to be as pure as that which is commonly uſed in all experiments, and therefore the fitteſt for my purpoſe.

To 2519,75 gr. of this oil of vitriol, whoſe ſpecific gravity was 1,819, I gradually added 180 gr. of diſtilled water, and ſix hours after found its ſpecific gravity to be 1,771.

To this mixture I again added 178,75 gr. of water, and found its ſpecific gravity, when cooled to the temperature of the atmoſphere, to be 1,719; it was then milky.

I then ſaturated the ſame quantity of the oil of tartar above mentioned with each of theſe ſorts of oil of vitriol in the manner already mentioned, and found the ſaturation to be effected (taking the medium of five experiments) by 6,5 gr. of that whoſe ſpecific gravity was 1,819; by 6,96 gr. of that whoſe ſpecific gravity was 1,771; and by 7,41 of that whoſe ſpecific gravity was 1,719.

I was obliged to add a certain proportion of water to each of theſe ſorts of oil of vitriol; for when they were not diluted, I perceived that part of the acid was phlogiſticated, and went off with the fixed air; but knowing the quantity of water that was added, it was eaſy to find, by the rule of proportion, the quantity of each ſort of oil of vitriol that was taken up by the alkali.

Hence I ſuppoſed, that each of theſe quantities of oil of vitriol of different denſities contained 3,55 gr. of acid, as they ſaturated the ſame quantity of vegetable fixed alkali as 11 gr. of ſpirit of nitre, which contained that quantity of acid.

I then endeavoured to find the ſpecific gravity of the pure vitriolic acid in the ſame manner as I before had that of the nitrous, as it cannot be had in the ſhape of air unleſs united to ſuch a quantity of phlogiſton as quite alters its properties. The loſs of 6,5 gr. of oil of vitriol, whoſe ſpecific gravity is 1,819 is $\frac{6{,}5}{1{,}819} = 3{,}572$; but as theſe 6,5 gr. contained, beſides 3,55 gr. acid, 2,95 of water, the loſs of this muſt be ſubtracted from the intire loſs, and then the remainder 0,622 is the loſs of the pure *acid* part in that ſtate of denſity to which it is reduced by its union with water. The ſpecific gravity therefore of the pure vitriolic acid in this ſtate of denſity is $\frac{3{,}55}{0{,}622} = 5{,}707$. But to find its natural ſpecific gravity we muſt find how much its denſity is increaſed by its union with this quantity of water: and, in order to obſerve this, I proceeded as before with the nitrous acid. 6,96 gr. of oil of vitriol, whoſe ſpecific gravity was 1,771, contained 3,55 gr. acid, and 3,41 of water; then its ſpecific gravity by calculation ſhould be 1,726, for the loſs of 3,55 gr. acid is $\frac{3{,}55}{5{,}707} = 0622$; the loſs of 3,41 gr. water is 3,41; the ſum of the loſſes 4,032. Then, $\frac{6{,}96}{4{,}032} = 17{,}261$; therefore the *accrued* denſity is 1,771 − 1,726 = ,045. Taking this therefore from 1,819, its mathematical ſpecific gravity will be 1,774, then the loſs of 6,5 gr. of oil of vitriol, whoſe ſpecific gravity by obſervation is 1,819, will be found to be $\frac{6{,}5}{1{,}774}=3{,}664$; but of this 2,95 gr. are the loſs of the water it contains, and the remainder 0,714 * are the loſs of the mere acid part. Then, $\frac{3{,}55}{0{,}715}=4{,}9649$ is nearly the true ſpecific gravity of the pure vitriolic acid.

I then found the true increaſe of denſity ariſing from the union of the vitriolic acid and water in the foregoing mixtures, and obſerved, that in oil of vitriol, whoſe ſpecific gravity was 1,771, it was 0,84, and in that whoſe ſpecific gravity was 1,719, it was 0,100.

To obtain a ſynthetical proof of theſe deductions, I compared them with the ſpecific gravities of the firſt mixtures I had made; for if theſe deductions were true, the mathematical ſpecific gravities, and the *accrued* denſities, added to each other, ſhould amount to the ſame quantity as the ſpecific gravities by obſervation; and this I found to happen very nearly; for in the firſt experiment, where 2519,75 gr. of oil of vitriol, whoſe ſpecific gravity was 1,819, were mixed with 180 gr. of water, that oil of vitriol contained by my calculation 1376,171 gr. of acid and 1143,597 gr. of water, beſides the 180 gr. of water that were added to it, the loſs of the acid was $\frac{1376{,}171}{4{,}964}=277{,}22$. The whole quantity of oil of vitriol was 2699,75 gr.; then the ſum of the loſſes was 1600,81; and therefore the mathematical ſpecific gravity $\frac{2699.75}{1600{,}81}=1{,}686$; to which, adding ,084 the degree of accrued denſity, the ſpecific gravity by obſervation ſhould be 1,770, which wants leſs than 1000th part in 2700 of being juſt.

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* By miſtake, the following calculations were made on the ſuppoſition that the loſs was 0,715; the difference being immaterial, the calculations were not repeated.

Again: in the mixture, whoſe ſpecific gravity was 1,719 the ſum of the loſſes was 1779,549, and the weight of the whole 2878,4, the mathematical ſpecific gravity ſhould be $\frac{2778,400}{1779,549} = 1,617$, to which adding 0,100, the ſpecific gravity by obſervation ſhould be 1,717, which is nearly the truth.

By continuing theſe mixtures until the ſpecific gravities by calculation and obſervation nearly coincided, I formed the following table. The extra-tabular proportions are to be ſought in the manner already ſhewn; the two firſt ſeries were formed by analogy.

On

Oil and pirit of vitriol.	Acid.	Water.	Accrued denſity.	Mathematical ſpecific gravity.	Specific gravity by obſervation	Attract. of the acid to water.	Attract. of water to the acid.
Grs.	Grs.	Grs.					
5,58	- -	2,03	,000	2,032	2,032	,005	0,140
6,04	- -	2,49	,005	1,884	1,889	,045	0,149
6,5	- -	2,95	,045	1,774	1,819	,084	0,139
6,96	- -	3,41	,084	1,687	1,771	0,100	0,137
7,41	- -	3,86	0,100	1,619	1,719	0,112	0,129
7,87	- -	4,32	0,112	1,563	1,675	0,122	0,122
8,33	- -	4,78	0,122	1,515	1,637	0,129	0,112
8,79	- -	5,24	0,129	1,476	1,605	0,137	0,100
9,25	- -	5,70	0,137	1,441	1,578	0,139	,084
9,71	- -	6,16	0,139	1,412	1,551	0,149	,045
10,17	- -	6,62	0,140	1,385	1,525	,0140	,005
10,63	- -	7,08	0,139	1,363	1,502		
11,09	- -	7,54	0,132	1,343	1,475		
11,55	- -	8,00	0,127	1,325	1,452		
12,01	- -	8,46	0,120	1,308	1,428		
12,47	- -	8,92	0,113	1,294	1,407		
12,93	- -	9,38	0,106	1,280	1,386		
13,39	- -	9,84	0,100	1,268	1,368		
13,85	- -	10,30	0,094	1,257	1,351		
14,31	- -	10,76	0,088	1,247	1,335		
14,77	- -	11,22	0,83	1,237	1,320		
15,23	- -	11,68	,078	1,228	1,306		
15,69	3,55	12,14	,074	1,220	1,294		
16,15	- -	12,60	,070	1,212	1,282		
16,61	- -	13,06	,066	1,205	1,271		
17,07	- -	13,52	,062	1,199	1,261		
17,53	- -	13,98	,059	1,191	1,250		
17,99	- -	14,44	,056	1,187	1,243		
18,45	- -	14,90	,053	1,181	1,234		
18,91	- -	15,36	,050	1,176	1,226		
19,37	- -	15,82	,047	1,167	1,214		
19,83	- -	16,28	,044	1,166	1,210		
20,29	- -	16,74	,040	1,162	1,203		
20,75	- -	17,20	,038	1,158	1,196		
21,21	- -	17,66	,035	1,154	1,189		
21,67	- -	18,12	,032	1,150	1,182		
22,13	- -	18,58	,029	1,146	1,175		
22,59	- -	19,04	,026	1,143	1,169		
23,05	- -	19,50	,023	1,140	1,163		
23,51	- -	19,96	,020	1,137	1,157		
23,97	- -	20,42	,018	1,134	1,152		
24,43	- -	20,88	,016	1,131	1,147		
24,89	- -	21,34	,014	1,128	1,142		
25,35	- -	21,80	,012	1,125	1,137		
25,81	- -	22,26	,010	1,123	1,133		
26,27	- -	22,72	,008	1,120	1,128		
26,73	- -	23,18	,006	1,118	1,124		
27,19	- -	23,64	,004	1,116	1,120		
27,65	- -	24,10	,002	1,114	1,116		

The ſpecific gravity of the moſt concentrated oil of vitriol yet made is, according to Mr. BAUME and BERGMAN, 2,125.

I aſcertained the proportion of acid water and fixed alkali in tartar vitriolate, as before, in nitre and digeſtive ſalt. I found the ſalts, reſulting from the ſaturation of the ſame oil of tartar, with portions of oil of vitriol of different ſpecific gravities, to weigh, at a medium, 12,45 gr. Of this weight only 11,85 gr. were alkali and acid, the remainder therefore was water, *viz.* 0,6 of a grain; conſequently 100 gr. of perfectly dry tartar vitriolate contain 28,51 gr. acid, 4,82 of water, and 66,67 of fixed vegetable alkali. Note, in drying this ſalt I uſed a heat of 240° to expel the adhering acid more thoroughly. I kept it in that heat a quarter of an hour.

According to Mr. HOMBERG, 1 French oz. (or 472,5 gr. Troy) of dry ſalt of tartar required 297,5 gr. Troy of oil of vitriol, whoſe ſpecific gravity was 1,674, to ſaturate it; but, by my calculation, this quantity of fixed alkali would require 325 gr.: a difference which, conſidering our different methods of determining the ſpecific gravity of liquids (his method, *viz.* that by menſuration, giving it always leſs than mine) the different deſiccation of our alkalies, &c. may paſs for inconſiderable.

The reſulting ſalt weighed, according to Mr. HOMBERG, 182 gr. Troy above the original weight of the fixed alkali; but by my experiment it ſhould weigh but 87,7 gr. more; for ∷ 10,5 . 12,45 :: 472,5 . 560,2. It is hard to ſay how Mr. HOMBERG could find this great exceſs of weight both in nitre and tartar vitriolate, unleſs he meant by the *original weight* of the ſalt of tartar the weight of the mere alkaline part, diſtinct from the fixed air it contained: and indeed one would be tempted to think, he did make this diſtinction; for in that caſe the exceſs of weight will be very nearly ſuch as he determined it: for ∺ 10,5 . 8,3 :: 472,5 . 373,3. Now, the whole weight of his nitre was 560,2, as I have above ſhewn: then 560,2 − 373,3 = 116,9, which is only 4 gr. more than he determined it.

Hence he inferred, that 1 oz. (472,5 gr. Troy) of this oil of vitriol contains 291,7 gr. of acid. By my computation it contains but 213,3; but it muſt be conſidered, he made no allowance for the water contained in tartar vitriolate, and imagined the whole of the increaſe of weight proceeded from the acid that is united in it to the fixed alkali. Now the aqueous part in 560 gr. of tartar vitriolate amounts to 37 gr. the remaining difference may be attributed to the different degrees of deſiccation, &c.

OF THE ACETOUS ACID.

I have made no experiment on this acid; but, by calculating from the experiment of Mr. HOMBERG, I find the ſpecific gravity of the pure acetous acid, free from ſuperfluous water, ſhould be 2,130. It is probable, its affinity to water is not ſtrong enough to cauſe any irregular increaſe in its denſity, at leaſt that can be expreſſed by three decimals; and hence its proportion of acid and water may always be calculated from its ſpecific gravity and abſolute weight.

100 parts of foliated tartar, or (as it ſhould rather be called) acetous tartar, contain well dried 32 of fixed alkali, 19 of acid, and 49 parts of water.

The ſpecific gravity of the ſtrongeſt concentrated vinegar yet made is 1,069.

It is harder to find the point of ſaturation with the vegetable than with the mineral acids; becauſe they contain a mucilage that prevents their immediate union with alkalies, and hence they are commonly uſed in too great quantity. They ſhould be uſed moderately hot, and ſufficient time allowed them to unite.

From theſe experiments it follows:

1ſt. That fixed vegetable alkalies take up an equal quantity of the three mineral acids, and probably of all pure acids; for we have ſeen, that 8,3 grains of pure vegetable alkali (that is, free from fixed air) take up 3,55 gr. of each of theſe acids, and conſequently 100 parts of cauſtic fixed alkali would require 42,4 parts of acid to ſaturate them. Now, Mr. BERGMAN has found, that 100 parts of cauſtic fixed vegetable alkali take up 47 parts of the aerial acid, which, conſidering his alkali might contain ſome water, differs but little from my calculation. It ſhould therefore ſeem, that alkalies have a certain determinate capacity of uniting to acids, that is, to a given weight of acids; and that this capacity is equally ſatiated by that given weight of any pure acid indiſcriminately. This weight is about 2,35 of the weight of the vegetable alkali.

2dly. That the three mineral acids, and probably all pure acids, take up 2,253 times their own weight of pure vegetable alkali, that is, are ſaturated by that quantity.

3dly. That the denſity accruing to compound ſubſtances from the union of their component parts, and exceeding its mathematical ratio, increaſes from a *minimum*, when the quantity of one of them is very ſmall in proportion to that of the other, to a *maximum*, when their quantities differ leſs; but that the attraction, on the contrary, of that part which is in the ſmalleſt quantity to that which is in the greater, is at its *maximum* when the accrued denſity is at its *minimum*, but not reciprocally; and hence the point of ſaturation is probably the *maximum* of denſity and the *minimum* of ſenſible attraction of one of the parts. Hence no decompoſition operated by means of a ſubſtance that has a greater affinity with one part of a compound than with the other, and than theſe parts have to each other, can be complete, unleſs the *minimum* affinity of this third ſubſtance be greater than the *maximum* affinity of the parts already united. Hence few decompoſitions are complete without a double affinity intervenes; and hence the laſt portion of the ſeparated ſubſtance adheres ſo obſtinately to that to which it was firſt united, as all chemiſts have obſerved. Thus, though acids have a greater affinity to phlogiſton than the earths of the different metals have to it, yet they can never totally dephlogiſticate theſe earths but only to a certain degree; ſo though atmoſpheric air, and particularly dephlogiſticated air, attracts phlogiſton more ſtrongly than the nitrous acid does; yet not even dephlogiſticated air can deprive the nitrous acid totally of its phlogiſton, as is evident from the red colour of the nitrous acid when nitrous air and dephlogiſticated air are mixed together. Hence alſo mercury precipitated from its ſolution in any acid, even by fixed alkalies, conſtantly retains a portion of the acid to which it was originally united, as Mr. BAYEN has ſhewn; ſo alſo does the earth of allum, when precipitated in the ſame manner from its ſolution; and thus ſeveral anomalous decompoſitions may be explained. Indeed, I have reaſon to doubt, whether mercury does not attract acids more ſtrongly than alkalies attract them.

4thly. That concentrated acids are, in ſome meaſure, phlogiſticated, and evaporate by union with fixed alkalies.

5thly. That, knowing the quantity of fixed alkali in oil of tartar, we may determine the quantity of real pure acid in any other acid ſubſtance that is difficultly decompoſed, as the ſedatif acid, and thoſe of vegetables and animals; for 10,5 gr. of the mild alkali will always be ſaturated by 3,55 gr. of real acid: and reciprocally, the quantity of acid in any acid liquor being known, the quantity of real alkali in any vegetable alkaline liquor may be found.

OF THE SPECIFIC GRAVITY OF FIXED AIR IN ITS FIXED STATE.

Being deſirous to know the ſpecific gravity of ſome ſubſtances which are difficultly procured, or at leaſt preſerved for any time, free from fixed air, ſuch as fixed and volatile alkalies, I was induced to ſeek the ſpecific gravity of the former in its fixed ſtate as of an element neceſſary to the calculation of the latter; it being very evident, that its denſity, in its fixed ſtate, muſt be very different from that which it poſſeſſes in its fluid elaſtic ſtate.

I therefore took a piece of white marble, of the pureſt kind, which weighed 440,25 gr. and weighing it in water, found it to loſe 162 gr.; its ſpecific gravity was therefore 2,7175.

Of this marble, reduced to a fine powder, I put 180 gr. into a phial, and expelling the fixed air by the dilute vitriolic acid and heat, I found its quantity amount to 105,28 cubic inches; the thermometer being at 65°, and the barometer between 29 and 30 inches, this bulk of air would, at 55° of FAHRENHEIT, occupy but 102,4 cubic inches; at which temperature, according to the experiment of Mr. FONTANA, a cubic inch of fixed air (the barometer being at 29°½) would weigh 0,57 of a grain; therefore the weight of the whole quantity of fixed air amounted to 58,368 gr. which is nearly one third of the weight of the marble. At this rate, 100 gr. of the marble contained 32,42 of fixed air.

To determine the proportion of water and calcareous earth, and alſo the ſpecific gravity of this latter, I put 3009,25 gr. of the ſame marble finely powdered into a crucible, looſely covered; the crucible and its contents, before calcination, weighed 8394 gr. and after remaining fourteen hours in a white heat I found it to weigh 7067,5 gr. The weight of the crucible alone was 5384,75 gr.; therefore the weight of the lime ſingly was 1682,75 gr. The marble then loſt by calcination 1326,5 gr.; 180 gr. of the marble ſhould then loſe 79,343 gr. and 100 gr. ſhould loſe 44,08; but of theſe 44,08, 32,42 were fixed air, as is already ſeen, therefore the remainder, that is, 11,66 gr. were water, and the quantity of pure calcareous earth in 100 gr. of the marble was 55,92 gr.

I next proceeded to diſcover the ſpecific gravity of the lime.

Into a braſs box, which weighed 607,65 gr. and in the bottom of which a ſmall hole was drilled, I ſtuffed as much as poſſible of the finely powdered lime, and then ſcrewed the cover on, and weighed it both in air and water. When immerſed in this latter, a conſiderable quantity of common air was expelled; when this ceaſed, I weighed it. The reſult of this experiment was as follows:

	Gr.
Weight of the box in air -	607,65
Its loſs of weight in water -	73,75
Weight of the box and lime in air	1043,5
Weight of the lime ſingly in air -	435,85
Loſs of weight of the box and lime in water	256,5
Loſs of weight of the lime ſingly -	182,3

Hence, dividing the abſolute weight of the lime by its loſs in water, its ſpecific gravity was found to be 2,3908.

From theſe *data* I deduced the ſpecific gravity of fixed air in its fixed ſtate; for 100 gr. of marble conſiſt of 55,92 of earth, 32,42 of fixed air, and 11,66 of water; and the ſpecific gravity of the marble is 2,717. Now, the ſpecific gravity of the fixed air, in its fixed ſtate, is as its abſolute weight divided by its loſs of weight in water; and its loſs of weight in water is as the loſs of 100 gr. of marble *minus* the loſſes of the pure calcareous earth and of the water.

$$\text{Loſs of 100 gr. of marble} = \frac{100}{2,717} = 36,8 \text{ gr.}$$

$$\text{Loſs of 55,92 gr. calcareous earth} = \frac{55,92}{2,390} = 23,39 \text{ gr.}$$

$$\text{Loſs of 11,66 gr. water} = \underline{11,66}$$

$$35,05$$

Then the loſs of the fixed air $36,8 - 35,05 = 1,75$; conſequently, its ſpecific gravity is $\frac{32,42}{1,75} = 18,52$; by which it appears to be the heavieſt of all acids, or even of all bodies yet known, gold and platina excepted.

OF FIXED VEGETABLE ALKALI.

As the manner of conducting the experiments I made on this ſalt was nearly the ſame as that I uſed in the foregoing (except that to find its ſpecific gravity I weighed it in æther inſtead of water), I ſhall content myſelf, to avoid the repetition of tedious calculation, with relating the reſult of theſe experiments.

1ſt. I found that 100 gr. of this alkali contain about 6,7 gr. of earth, which, according to Mr. BERGMAN, is ſiliceous: this earth paſſes the filter with it when the alkali is not ſaturated with fixed air, ſo that it ſeems to be held in ſolution as in *liquor ſilicum.*

2dly. I found, that the quantity of fixed air in oil of tartar and dry vegetable fixed alkali is various at various times and in various parcels of the ſame ſalt; but that at a medium in the purer alkalies it may be rated at 21 gr. in 100; and hence the quantity of this alkali in any ſolution of it may be very nearly gueſſed at, by adding a known weight of a dilute acid to a given weight of ſuch ſolution, and then weighing it again; for as 21 is to 100, ſo is the weight loſt to the weight of mild alkali in ſuch ſolution.

The ſpecific gravity of mild and perfectly dry four times calcined fixed alkali, free from ſiliceous earth, and containing 21 *per cent.* of fixed air, I found to be 5,0527.

When it contains more fixed air, its ſpecific gravity is probably higher, except it were not perfectly dry: from whence I inferred the ſpecific gravity of this alkali, when cauſtic and free from water, to be 4,234.

From the weight of the aerial acid, in its fixed ſtate, it happens, that fixed alkalies, when united to it, are ſpecifically heavier than when united either to the vitriolic or nitrous acids. Thus Mr. R. WATSON, in the Phil. Tranſ. for the year 1770, p. 337. found the ſpecific gravity of dry ſalt of tartar (including ſiliceous earth) to be 2,761: whereas the ſpecific gravity of tartar vitriolate was only 2,636, and that of nitre 1,933. The reaſon why nitre is ſo much lighter than tartar vitriolate, is, becauſe it contains much more water, and its union with the alkali is leſs intimate.

Laſtly, I have drawn up a table of the quantity of mild alkali, containing 6,7 *per cent.* of earth (which is its uſual degree of purity) to be found in natural or artificial ſolutions of this alkali, the thermometer at 63°; and though it is not quite accurate, wanting about 1,1 *per cent.* of the truth, yet, I preſume, it may be found uſeful, as this error is eaſily corrected.

Table of the contents of a ſolution of mild vegetable alkali, according to its ſpecific gravity.

Gr. of the ſolution.	Gr. of alkali.	Gr. of water.	Accrued denſity.	Mathematical ſpecific gravity.	Specific gravity by obſervation
64,92	- -	38,67	,050	1,445	1,495
70,60	- -	44,35	,049	1,393	1,446
76,28	- -	50,03	,048	1,356	1,404
81,96	- -	55,71	,047	1,324	1,371
87,64	- -	61,39	,046	1,297	1,343
93,32	- -	67,07	,045	1,274	1,319
99,00	- -	72,75	,044	1,254	1,298
104,68	- -	78,43	,043	1,237	1,280
110,36	- -	84,11	,042	1,223	1,265
115,98	- -	89,79	,041	1,209	1,250
121,66	- -	95,4	,040	1,198	1,238
127,34	- -	101,15	,039	1,187	1,226
133,02	- -	106,83	,038	1,178	1,216
138,7	- -	112,51	,037	1,170	1,207
144,3	- -	118,19	,036	1,162	1,198
149,98	- -	123,87	,035	1,155	1,190
155,66	- -	129,55	,034	1,149	1,183
161,34	- -	135,23	,033	1,143	1,176
167,02	26,25	140,91	,032	1,138	1,170
172,70	- -	146,59	,031	1,132	1,163
178,38	- -	152,27	,030	1,128	1,158
184,06	- -	157,95	,029	1,123	1,152
189,74	- -	163,63	,028	1,119	1,147
195,42	- -	169,31	,027	1,115	1,142
201,10	- -	174,99	,026	1,112	1,138
206,78	- -	180,67	,025	1,108	1,133
212,46	- -	186,35	,024	1,105	1,129
218,14	- -	192,03	,023	1,100	1,123
223,82	- -	197,71	,022	1,099	1,121
229,50	- -	203,39	0,21	1,097	1,118
235,18	- -	209,07	,020	1,094	1,114
240,86	- -	214,75	0,19	1,092	1,111
246,54	- -	220,43	,018	1,089	1,107
252,12	- -	226,11	0,17	1,087	1,104
257,80	- -	231,79	,016	1,085	1,101
263,48	- -	237,47	,015	1,083	1,098
269,16	- -	243,15	,014	1,081	1,095

Gr. of the ſolution.	Gr. of alkali.	Gr. of water.	Accrued denſity.	Mathematical ſpecific gravity.	Specific gravity by obſervation
274,78	- -	248,83	,013	1,079	1,092
280,46	- -	254,51	,012	1,077	1,089
286,14	- -	260,19	,011	1,076	1,087
291,82	- -	265,87	,010	1,074	1,084
297,50	- -	271,55	,009	1,070	1,079
303,18	- -	277,23	,008	1,069	1,077
308,86	26,25	282,91	,007	1,068	1,075
314,54	- -	288,59	,006	1,066	1,072
319,22	- -	294,27	,005	1,065	1,070
324,90	- -	300,45	,004	1,064	1,068
330,58	- -	306,13	,003	1,063	1,066
336,26	- -	311,81	,002	1,062	1,064
341,94	- -	317,49	,001	1,061	1,062

Impure vegetable fixed alkalies ſuch as pearl aſh, pot aſhes, &c. contain more fixed air, as appears by the experiments of Dr. LEWIS. Pearl aſh, according to Mr. CAVENDISH, contains 28,4 or 28,7 *per cent.* of fixed air. Hence in lyes of equal ſpecific gravity with thoſe of a purer alkali, the quantity of ſaline matter will be more probably in the ratio of 28,4 or 28,7 to 21; but this ſurplus weight is only fixed air; and hence even in theſe lyes the quantity of depurated ſalt they will afford will be found by the above table. Much alſo depends on their age, the oldeſt containing moſt fixed air.

III. *Account of the violent Storm of Lightning at* Eaſt-bourn, *in* Suſſex, Sept. 17, 1780; *communicated by* Owen Saluſbury Brereton, *Eſq. F. R. and A. S.*

Read December 14, 1780.

SIR,

I AM deſired by my friend and neighbour JAMES ADAIR, Eſq. of Soho Square, to communicate to you an account of the dreadful accident which happened to him and his family at Eaſt-bourn, in the county of Suſſex, at nine o'clock in the morning, on Sunday the ſeventeenth of September laſt. He rented a houſe which ſtood by itſelf; built of various ſorts of ſtone, three ſtories high, and facing the ſea, which was nearly South-eaſt of it. The morning was very ſtormy, with rain, thunder, and lightning; and juſt at nine o'clock a horrid black cloud appeared, out of which Mr. ADAIR ſaw ſeveral balls of fire drop into the ſea ſucceſſively, as he was approaching the window in a one-pair of ſtairs room; and very ſoon after, as he was ſtanding at it with his hands claſped, and extended open againſt the middle of the frame, a moſt violent flaſh of fire forced his hands aſunder, and threw him ſeveral yards upon the floor on his back, with both his legs upright in the air, which remained long ſo fixed. He was very ſenſible of his ſituation all the time, but could not open his eyes or ſpeak; nor had he the leaſt power of motion of any of his limbs for a long time. On help coming in, and examining his cloaths, which were blue cloth, his right ſleeve, both of coat and waiſtcoat, and alſo ſhirt, were all torn on the inſide of the arm entirely open, as if by a dog, from the ſhoulder to the wriſt; the right-ſide of the breeches was torn in the ſame manner, and part of each of the braſs buttons melted.

He had in his fob a gold watch with a ſteel chain; the button which opens it and three other places of the caſe were melted. The pendant to which the chain is fixed was almoſt melted through, and much of the ſteel chain is incorporated with it, as is reciprocally ſome gold on that part of the ſteel which was within the fob. The going of the watch had ſtopped inſtantaneouſly, occaſioned, as at firſt appeared by the ſmall pendulum ſpiral ſteel ſpring having been lengthened; not that it was abſolutely ſo, but relatively, reſpecting the ſcapement of the watch, the ſeveral inner turns being brought cloſer together.

His right-arm, right-ſide, and thigh, were miſerably ſcorched, and the fleſh torn: the foot of the ſtocking of his right leg and his ſhoe were torn in ſeveral places between the buckle and the toe-end of the ſhoe, and one of his toes ſplit almoſt to the bone; but the buckle, which was a broad ſilver one, was not the leaſt hurt or even marked, and remained buckled as before. His ſleeve-button of gold, in which was platted hair covered with cryſtal, was broken from its link, and neither hair or cryſtal have been found ſince. A key and a pen-knife in his right-ſide breeches pocket have ſeveral marks of fuſion upon them.

The frame of the window on which Mr. ADAIR was leaning was little damaged; but every pane of glaſs ſo completely ſmaſhed, you could ſcarce perceive it had ever had glaſs in it. The room was ſtuccoed and papered, and between the windows hung a large pier glaſs, which, with much of the ſtucco, was ſhivered to pieces, and ſtrewed over the floor. A door oppoſite the window was ſhattered to pieces, and the poſts of a bed in a room behind it, and all the bell-wires were deſtroyed.

Under the dining-room Mr. ADAIR was in, on the parlour floor, were his coachman, butler, and footman. The coachman was going to open a glaſs-door to go towards the ſea, and was ſtruck dead. His body was totally black. His cloaths, and the caul of his wig, and cravat, were much torn; but no particular fleſh wound was found. The enamelled face of his ſilver watch was broken to pieces, and the links of his ſteel chain faſtened together.

The footman was dreſſing his hair near a window, when he was thrown dead on the ground. He appeared much ſcorched, bruiſed, and black. He had a very large wound in his ſide which penetrated near his heart; but very little, if any, blood came from it. His buck-ſkin breeches were much torn, and the ſteel of a metal knee-buckle driven through them. The window ſaſh was driven into the room, and a ſtone, about eight inches ſquare, forced out of the wall into the middle of the room, not far from the body. The butler was a yard or two behind the coachman, and going out with a teleſcope in his hand, which was forced in pieces from him, his hat and wig were thrown to ſome diſtance, and he perceived a violent preſſure on his ſkull and on his back, but was no otherwiſe hurt. He had a ſilver watch with a ſilver chain, which received no damage.

In the room over Mr. ADAIR's, a young lady was dreſſing, and her maid attending. They were both driven to a diſtant part of the room, and rendered inſenſible for ſome time, but not hurt. The poſts of the bed ſhe had juſt left were all ſhivered to pieces, and the bell wires deſtroyed, and the chimney thrown down on the roof.

It is to be remarked, that though the bodies of the two ſervants lay unburied from the Sunday till Tueſday, all their limbs were as entirely flexible as thoſe of a living perſon.

Multitudes on the ſhore before the houſe ſaw the meteor dart in a right line over their heads, and break againſt the front of the houſe in different directions, and all agreed the form and flame was exactly like that of an immenſe ſky-rocket.

IV. *An Account of the* Harmattan, *a ſingular* African *Wind. By* Matthew Dobſon, *M. D. F. R. S.*; *communicated by* John Fothergill, *M. D. F. R. S.*

Read December 7, 1780.

THE Harmattan is a periodical wind which blows from the interior parts of Africa towards the Atlantic Ocean, and poſſeſſes ſuch extraordinary properties, as to merit the attention of the naturaliſt, making a curious and important article in the hiſtory and theory of the winds.

The firſt information I had on this ſubject was from my friend Mr. NORRIS, who has frequently viſited the Coaſt of Africa, and is a gentleman of an excellent underſtanding and ſtrict veracity. This information immediately excited my attention; and as Mr. NORRIS was preparing to make another voyage to that part of the world, I deſired him to confirm the facts which he had related, by further inquiries, experiments, and obſervations; and it is from theſe materials, with which I have been obligingly furniſhed by Mr. NORRIS, that the following account is drawn up.

On that part of the Coaſt of Africa which lies between Cape Verd and Cape Lopez, an eaſterly wind prevails during the months of December, January, and February, which by the Fantees, a nation on the Gold Coaſt, is called the Harmattan. Cape Verd is in 15° N. latitude, and Cape Lopez in 1° S. latitude, and the coaſt between theſe two Capes runs, in an oblique direction, nearly ftom W.S.W. to E.S.E. forming a range of upwards of two thouſand one hundred miles. At the Iſles de Los, which are a little to the northward of Sierra Leone, and to the Southward of Cape Verd, it blows from the E.S.E. on the Gold Coaſt from the N.E. and at Cape Lopez and the River Gabon from the N.N.E. This wind is, by the French and Portugueze who frequent the Gold Coaſt, called ſimply the N.E. wind, the quarter from which it blows. The Engliſh, who ſometimes borrow words and phraſes from the Fantee language, which is leſs guttural and more harmonious than that of their neighbours, adopt the Fantee word Harmattan.

The Harmattan comes on indiſcriminately at any hour of the day, at any time of the tide, or at any period of the Moon, and continues ſometimes only a day or two, ſometimes five or ſix days, and it has been known to laſt fifteen or ſixteen days. There are generally three or four returns of it every ſeaſon. It blows with a moderate force, not quite ſo ſtrong as the ſea breeze (which every day ſets in during the fair ſeaſon from the W. W.S.W. and S.W.); but ſomewhat ſtronger than the land wind at night from the N. and N.N.W.

1. *A fog or haze* is one of the peculiarities which always accompanies the Harmattan. The gloom occaſioned by this fog is ſo great, as ſometimes to make even near objects obſcure. The Engliſh fort at Whydah ſtands about the midway between the French and Portugueſe forts, and not quite a quarter of a mile from either, yet very often from thence neither of the other forts can be diſcovered. The ſun, concealed the greateſt part of the day, appears only about a few hours about noon, and then of a mild red, exciting no painful ſenſation on the eye.

As the particles which conſtitute the fog are depoſited on the graſs, the leaves of trees, and even on the ſkin of the negroes, ſo as to make them appear whitiſh, I recommended to Mr. NORRIS the uſe of a good microſcope, as this might poſſibly diſcover ſomething concerning the nature of theſe particles. "I was prevented," ſays Mr. NORRIS, "by the bad ſtate of "my health from availing myſelf of the microſcope; neither "could I diſcover any thing by the taſte, or by expoſing plates "covered thinly with melaſſes, for when I had dropped an acid "or alkali into the water in which I had diſſolved the melaſſes, "nothing followed to enable me to judge of the nature "of the particles. Surely they cannot be inſects, or animalculæ of inſects? for we have no appearance of any "thing produced from the myriads of them which are depoſited "on the earth. They do not flow far over the ſurface of the "ſea: at two or three miles diſtance from the ſhore the fog is "not ſo thick as on the beach; and at four or five leagues diſtance it is intirely loſt, though the Harmattan itſelf is plainly "felt for ten or twelve leagues, and blows freſh enough to alter "the courſe of the current."

2. *Extreme dryneſs* makes another extraordinary property of this wind. No dew falls during the continuance of the harmattan; nor is there the leaſt appearance of moiſture in the atmoſphere. Vegetables of every kind are very much injured; all tender plants, and moſt of the productions of the garden, are deſtroyed; the graſs withers, and becomes dry like hay; the vigorous ever-greens likewiſe feel its pernicious influence; the branches of the lemon, orange, and lime trees droop, the leaves become flaccid, wither, and, if the harmattan continues to blow for ten or twelve days, are ſo parched as to be eaſily rubbed to duſt between the fingers: the fruit of theſe trees, deprived of its nouriſhment, and ſtinted in its growth, only appears to ripen, for it becomes yellow and dry, without

acquiring half the uſual ſize. The natives take this opportunity of the extreme dryneſs of the graſs and young trees to ſet fire to them, eſpecially near their roads, not only to keep thoſe roads open to travellers, but to deſtroy the ſhelter which long graſs, and thickets of young trees, would afford to ſkulking parties of their enemies. A fire thus lighted flies with ſuch rapidity as to endanger thoſe who travel: in that ſituation a common method of eſcape is, on diſcovering a fire to windward, to ſet the graſs on fire to leeward, and then follow your own fire. There are other extraordinary effects produced by the extreme dryneſs of the Harmattan. The covers of books, Mr. NORRIS informs me, even cloſely ſhut up in a trunk, and lying among his cloaths, were bent as if they had been expoſed to the fire. Houſhold furniture is alſo much damaged: the pannels of doors and of wainſcot ſplit, and any veneered work flies to pieces. The joints of a well-laid floor of ſeaſoned wood open ſufficiently to lay one's finger in them; but become as cloſe as before on the ceaſing of the Harmattan. The ſeams alſo in the ſides and decks of ſhips are much injured and become very leaky, though the planks are two or three inches in thickneſs. Iron-bound caſks require the hoops to be frequently driven tighter; and a caſk of rum or brandy, with wooden hoops, can ſcarcely be preſerved; for, unleſs a perſon attends to keep it moiſtened, the hoops fly off.

The parching effects of this wind are likewiſe evident on the external parts of the body. The eyes, noſtrils, lips, and palate, are rendered dry and uneaſy, and drink is often required, not ſo much to quench thirſt, as to remove a painful aridity in the *fauces*. The lips and noſe become ſore, and even chapped; and though the air be cool, yet there is a troubleſome ſenſation of prickling heat on the ſkin. If the Harmattan continues

four

four or five days, the ſcarf ſkin peels off, firſt from the hands and face, and afterwards from the other parts of the body, if it continues a day or two longer. Mr. NORRIS obſerved, that when ſweat was excited by exerciſe on thoſe parts which were covered by his cloaths from the weather, it was peculiarly acrid, and taſted, on applying his tongue to his arm, ſomething like ſpirit of hart's-horn diluted with water.

As the ſtate of ſalt of tartar placed in the open air, and the quantity evaporated from a given ſurface of water, are obvious proofs of the comparative moiſture or dryneſs of the atmoſphere, I deſired Mr. NORRIS to put the Harmattan to each of theſe teſts; and particularly to moiſten ſalt of tartar *ad deliquium*, and expoſe it to the night air during the time that the Harmattan was blowing. The following is the account of the reſult of theſe experiments. Salt of tartar will not only remain dry during the night as well as in the day; but, when liquified ſo as to run upon a tile, and expoſed to the Harmattan, becomes perfectly dry in two or three hours; and, expoſed in like manner to the night air, will be dry before morning.

With reſpect to evaporation Mr. NORRIS ſays, "I fixed the "tin veſſel, with which you favoured me, on a graſs plat "behind my houſe, upon a ſtand four feet high, and expoſed "by its ſituation moſt part of the day to the ſun, but ſhel-"tered in ſome meaſure from the wind by the houſe."

Day

Day of the Month.	Evaporation of one tenth of an inch.		Thermometer 6 Morn.	1 Noon.	6 Even.	Remarks.
			°	°	°	Light breeze and hazy.
Nov. 27	1½		–	–	–	Ditto and fair weather.
28	2		–	–	–	Hazy with regular land and ſea breezes.
29	2		–	–	–	Ditto.
30	1½		–	–	–	Ditto.
Dec. 1	1½		–	–	–	Freſh breezes and fair.
2	2		76	80	79	
	Day.	Night.				
3	2	1	74	76	75	Harmattan began to blow moderately.
4	2½	1½	75	77	76	Harmattan ſtill blowing, but moderately, and the fog not conſiderable.
5	2	1	74	76	76	Harmattan almoſt over.
6	2		76	80	78	Harmattan over, ſea breeze as uſual, hazy.
7	2		76	80	78	Light breeze and hazy.
8	2		76	80	78	Ditto.

"The thermometer hung in a large warehouſe near a win-"dow on which the ſun never came at that ſeaſon of the year, "as it had a north aſpect, and where little reflected heat came, "a graſs plat being before it. When removed into the next "room, which had three windows and a door opening into the "parade, the thermometer uſually roſe 4° higher than it did in "the warehouſe; its general height in the room, from one to "three o'clock, was 84°.

"On the 14th of December, when there was no Harmattan, "the thermometer at noon, on putting it into the evaporating "veſſel, roſe to 88°; on taking it out, it ſunk to 79°, whilſt the "moiſture on its ſurface was evaporating; but on expoſing it "five minutes to the ſun it roſe to 102°. On the fifteenth of "December the thermometer, expoſed to the wind in my "room window, but not to the ſun, ſtood at noon at 84°; at "88° in the evaporating veſſel; ſunk to 80° as the water evaporated from its ſurface; roſe in the ſun in ſix minutes to "104°; and, on putting it into the water-jar in my room, "ſunk to 76°."

It appears from the preceding experiments made by Mr. NORRIS, that, if the evaporation of the whole year be ſuppoſed to go on in the ſame proportion with what occurred during a ſhort and very moderate return of the Harmattan, the annual Harmattan evaporation would be 133 inches; and if the calculation was made in proportion to the evaporation which occurs during a longer viſit from the Harmattan, and a more forcible breeze, the annual Harmattan evaporation would be much more conſiderable. If the annual evaporation be in like manner calculated, in proportion to the evaporation which took place ſubſequent to and preceding the Harmattan, the annual evaporation at Whydah on the Gold Coaſt would be 64 inches, and I have found the annual evaporation at Liverpool to be 36 inches*. Theſe three therefore are in the following proportion; Harmattan 133 inches, Whydah 64 inches, and Liverpool 36 inches.

As the names of things are often derived from ſome remarkable property in the thing denoted, I deſired Mr. NORRIS to inquire into the derivation of the word Harmattan. He found it to be a corruption of *Aberramantah*, the name of that ſeaſon in which this wind blows. Aherramantah is compounded of *Aberraman*, which in the Fantee language ſignifies to blow, and *tah*, tallow or greaſe, with which the natives rub their ſkins to prevent their growing dry and rough.

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* Philoſophical Tranſactions, vol. LXVII. p. 252.

The

The Harmattan ſeaſon is in the Dunco language called *Peppeh*, ſignifying a dry and rough ſkin.

3. *Salubrity* forms a third peculiarity of the Harmattan. Though this wind is ſo very prejudicial to vegetable life, and occaſions ſuch diſagreeable parching effects on the human ſpecies, yet it is highly conducive to health. Thoſe labouring under fluxes and intermitting fevers generally recover in an Harmattan. Thoſe weakened by fevers, and ſinking under evacuations for the cure of them, particularly bleeding, which is often injudiciouſly repeated, have their lives ſaved, and vigour reſtored, in ſpite of the doctor. It ſtops the progreſs of epidemics: the ſmall-pox, remittent fevers, &c. not only diſappear, but thoſe labouring under theſe diſeaſes when an Harmattan comes on, are almoſt certain of a ſpeedy recovery. Infection appears not then to be eaſily communicated even by art. In the year *1770* there were on board the Unity, at Whydah, above 300 ſlaves; the ſmall-pox broke out among them, and it was determined to inoculate; thoſe who were inoculated before the Harmattan came on got very well through the diſeaſe. About ſeventy were inoculated a day or two after the Harmattan ſet in; but not one of them had either ſickneſs or eruption. It was imagined, that the infection was effectually diſperſed, and the ſhip clear of the diſorder; but in a very few weeks it began to appear among thoſe ſeventy. About fifty of them were inoculated the ſecond time; the others had the diſeaſe in the natural way: an Harmattan came on, and they all recovered, except one girl, who had an ugly ulcer on the inoculated part, and died ſome time afterwards of a locked jaw. The great ſalubrity, and the power of checking epidemics, are ſuch extraordinary properties of the Harmattan, that I deſired Mr. NORRIS, on his next voyage to the Coaſt, to aſcertain theſe points by further

inquiries. "I have not much new," ſays Mr. NORRIS, "on "theſe points, ſave the general teſtimony of the natives in con-"firmation of what I have already communicated; and that I "had been very ill myſelf for nine days with a remittent fever "this voyage, of which I recovered immediately upon the Har-"mattan beginning to blow; whether from the medicines "which I had taken, or from the alteration in the ſtate of the "atmoſphere, I pretend not to determine. I now learned, for "the firſt time, that the Harmattan is noted for contributing "much to the cure of ulcers, as well as cutaneous eruptions." Mr. NORRIS is ſorry to be obliged to diſſent from ſo reſpectable an authority as that of Dr. LIND, who ſpeaks of the Harmattan as "fatal and malignant; that its noxious vapours are deſtruc-"tive to Blacks as well as Whites; and that the mortality "which it occaſions is in proportion to the denſity and duration "of the fog." The baneful effects here pointed out proceed from the periodical rains which fall in March, April, &c. and which are uſhered in by the Tornadoes, or ſtrong guſts of wind from the N.E. and E.N.E. accompanied with violent thunder and lightning, and very heavy ſhowers. The earth drenched by theſe ſhowers, and acted upon with an intenſe ſolar heat as ſoon as the ſtorm is over, ſends forth ſuch noiſome vapours as ſtrike the noſtrils with a moſt offenſive ſtench, and occaſion bilious vomitings, fluxes, and putrid fevers. Beſides theſe vapours, which are annual, there appears to be a collection of ſtill more peſtiferous matter, confined for a longer time, and iſſuing from the earth after an interval of five, ſix, or ſeven years. "The periods," ſays Mr. NORRIS, "which I remem-"ber to have been thus marked, were in 1756, when Governor "MELVILL and moſt of the gentlemen and ſoldiers at Cape "Coaſt, died; in 1763, 1769, and 1775. The mortality

" in ſome of theſe years, for they were not all equally fatal, " was ſo great that, as Dr. LIND ſays, the living were ſcarce " ſufficient to remove and bury the dead."

It is to be obſerved, that there may be inſtances in which the Harmattan comes loaded with the effluvia of a putrid marſh; and if there are any ſuch ſituations, the nature of the wind may be ſo changed as to become even noxious.

Another inquiry which I deſired Mr. NORRIS to make reſpected the ſource of the Harmattan, and the nature of the ſoil over which it blows. It appears that, except a few rivers and ſome lakes, the country about and beyond Whydah is covered for four hundred miles back with verdure, open plains of graſs, clumps of trees, and ſome woods of no conſiderable extent. The ſurface is ſandy, and below that a rich reddiſh earth; it riſes with a gentle aſcent for one hundred and fifty miles from the *ſea* before there is the appearance of an hill, without affording a ſtone of the ſize of a walnut. Beyond theſe hills there is no account of any great ranges of mountains.

With reſpect to the origin of this wind, Mr. NORRIS ſays, " the Harmattan, according to Dr. LIND, ariſes from the con" flux of ſeveral rivers about Benin; but when I was on a " viſit to the King of Dahomey, one hundred and twenty " miles North, or inland from the Fort at Whydah, I there " felt the Harmattan blowing from the N.E. ſtronger than I " have at any other time, though Benin then bore from me " S.E."

On this head Mr. NORRIS makes the following conjecture: " The interſection of three lines, *viz.* an eaſt line drawn from " Cape Verd, a north-eaſt one from the centre of the Gold " Coaſt, and a north line from Cape Lopez, would, I think, " point out a probable ſource of this extraordinary wind."

Three lines, drawn according to the direction of Mr. NORRIS, towards the points of the compaſs from which the Harmattan blows on Cape Verd, the Gold Coaſt, and Cape Lopez, converge I find to a part of Africa about the 15th degree of N. latitude, and the 25th degree of E. longitude, which I alſo find to be that part of Africa where, according to PTOLEMY, the mountains of Caphas are ſituated. From theſe mountains, according to the ſame authority, the river Daradus aroſe, ſuppoſed by ſome to be now the river Senegal.

It may be conjectured, that the diſagreeable Levant wind of the Mediterrannean proceeds from the ſame part of the Continent of Africa; for it prevails during the ſame ſeaſon of the year, and may derive its qualities from the ſurface over which it paſſes.

The laſt article of information with which I have been favoured by Mr. NORRIS, is an account of the manner in which the Fantee nation divide their year.

Aherramantah, from the 1ſt of December to the middle of February, about 10 weeks.

Quakorah, a wind up the coaſt, from S.S.W. to S.S.E. from the middle of February to the firſt week in March, about 3 weeks.

Pempina, or Tornado ſeaſon, part of March, all April, and the greateſt part of May, about 12 weeks.

Abrenama, or the old man's and woman's children, that is, the Pleiades, the rainy ſeaſon, the latter end of May, all June, and to about the 20th of July, 8 weeks.

Atukogan, or five ſtars, that is, Orion, high wind and ſqually, the rains very heavy, to the middle of Auguſt, 3 weeks.

Worrobakorou, or one ſtar, the ceaſing of the rains, about 3 weeks.

Mawurrah, the name of a certain ſtar; cloſe, foggy weather and no breeze, the firſt three weeks in September.

Boutch, no land breeze in this ſeaſon, the wind blows freſh down the Coaſt, about ſix weeks.

Autiophi, or the Croziers; Tornadoes and ſoutherly wind, with ſome rain, generally called the latter rains, about four weeks, to the beginning of December, when the Aherramantah ſeaſon again commences.

V. *Eſſay on a new Method of applying the Screw. By Mr.* William Hunter, *Surgeon; communicated by Lieutenant General* Melville, *F. R. S.*

Read December 21, 1780.

I HAVE ſome time ago been led to think, that the ſcrew, which of all the mechanical powers is the moſt commonly employed in performing motions which require great accuracy, might be applied in a manner which would better anſwer many intentions than that commonly uſed. The plan is ſomewhat ſimilar to NONIUS's diviſion of the circle; but before I explain myſelf farther it may be proper to lay down a few general rules on which we may found a compariſon.

The perfection of any machine conſiſts in accompliſhing the end propoſed in a manner the moſt effectual, the moſt expeditious, and the leaſt cumberſome poſſible. In order to attain this end the following things are required.

1. That the ſtrength of the ſeveral parts of the engine be ſo adjuſted to the force they are intended to exert, as that they ſhall not break under the weight they ought to counteract, nor yet encumber the motion by a greater quantity of matter than is neceſſary to give them a proper degree of ſtrength.

2. That the increaſe of power, by means of the machine, be ſo regulated, that while the force we can exert is thereby rendered adequate to the effect, it may not be retarded in procuring it more than is abſolutely neceſſary.

3. Th

3. That the machine be as ſimple as is conſiſtent with other conditions.

4. That it be as portable and as little troubleſome in the application as poſſible.

5. That the contrivance be ſuch that the moving power may be applied in ſuch a way as to act to the greateſt advantage; and that the motion ultimately produced may have that direction and velocity which is moſt adapted to the execution of the deſign propoſed by the whole.

6. Of two machines, equal in other reſpects, that deſerves the preference in which the friction leaſt diminiſhes the effect propoſed by the whole.

It will eaſily appear, that ſome of theſe conditions, if carried to an extreme, will be inconſiſtent with ſome of the others. Here the proper medium conſiſts in adapting them to each other in ſuch a manner, as that the reſult of the advantages of both may be the greateſt, and that of the defects the leaſt, that is poſſible.

The following method of applying the ſcrew, I think, may in certain caſes be attended with ſome of theſe advantages to a greater degree than by thoſe commonly practiſed.

Let AB (fig. 1.) be a plate of metal in which the ſcrew CD plays, having a number of threads in an inch equal to a. Within the ſcrew CD there is a female ſcrew, by which is received the ſmaller ſcrew DE of $a+1$ threads in an inch. This ſcrew is retained from moving round along with the ſcrew CD by means of the apparatus at AFGB.

Now, if the handle CKL be turned a times round the ſcrew, CD will advance upwards an inch, and if we ſuppoſe the ſcrew DE to move round along with CD, the point E will alſo advance an inch. If we now turn the ſcrew DE a times backwards, the point E will move downwards $\frac{a}{a+1}$ of an inch, and the reſult of both motions will be to lift the point E upward ($1-\frac{a}{a+1}=$) $\frac{1}{a+1}$ of an inch. But if, while the ſcrew CD is turned a times round, DE be kept from moving, the effect will be the ſame as if it had moved a times round with CD and been a times turned back, that is, it will advance $\frac{1}{a+1}$ of an inch. At one turn therefore of the handle CKL it will move upwards ($\frac{1}{a+1}\times\frac{1}{a}=$) $\frac{1}{a^2+a}$ of an inch. If then we ſuppoſe the handle CKL to be b inches long, the power gained by the machine will be as $\overline{a^2+a}\times 6{,}2832\,b$ to unity.

To illuſtrate this by a particular example, let the ſcrew CD have 10 threads in an inch, and DE 11: then, while the handle CKL is turned 10 times round, the point D will riſe one inch above its former ſituation. But at 10 turns it can only paſs over 10 threads of the ſcrew DE, and conſequently it will advance upon that ſcrew $\frac{10}{11}$ths of an inch. The point E therefore muſt riſe $\frac{1}{11}$th of an inch, that the point D may have room to riſe a complete inch above its former place: therefore, at one turn of the handle, the point E will riſe $\frac{1}{110}$th of an inch; and if the handle be ſuppoſed half a foot long, the power, to produce an equilibrium, muſt be to the weight as 1 to 110 × 6,2832 × 6 = 4146,912, which is the very number expreſſed by the general theorem, *viz.* $\overline{a^2+a}\times 6{,}2832\,b$, calling $a=10$ and $b=6$.

Now let us compare, according to the rules before laid down, this method of uſing the ſcrew with the common one. And, firſt, in order to have the ſame power by means of the common ſcrew that is exerted by this machine, it muſt have a number of threads in an inch equal to a^2+a, which would render it too weak to reſiſt any conſiderable violence. For example, if DO have five threads in an inch, and DE ſix, and if the handle CKL is a foot in length, the power gained by the engine will be nearly as $(\overline{a^2+a} \times 6b =)$ 2160 to 1; whereas, to have the ſame force by means of the common ſcrew, it muſt have 30 threads in an inch, and ſo muſt yield under a reſiſtance which the other ſcrew would overcome without any difficulty. Upon this principle, the ſcrew may be applied with advantage in preſſes of different kinds, by fixing one of the plates of the preſs to the end of the ſcrew at E.

As to the ſecond requiſite, both methods may be equally adapted to it; yet other circumſtances will determine us to apply the common ſcrew where a ſmall increaſe of power is neceſſary, and the preſent contrivance, when we ſtand in need of a greater.

This will follow from the third rule, as in the method now propoſed a double number of ſcrews is required, which makes the ſtructure more complicated, occaſions more expence, and requires a greater accuracy of conſtruction, ſince, unleſs this is attended to, the machine will not move.

However, the machine may, in ſome caſes, anſwer the fourth intention better than the common one, as the power gained by the additional ſcrew enables us to ſhorten the handle which will tend to make the whole more portable.

The power is here applied in the ſame direction as in the common ſcrew, ſo that both equally anſwer the firſt part of the fifth rule; but as to the laſt, the motion *ultimately* produced, it will depend on particular circumſtances which of them is moſt fit for uſe in any caſe. Thus, if the ſcrew DE be intended to carry an index which muſt turn round at the ſame time that it riſes upwards, the common ſcrew is preferable; for although I can ſee a method by which the machine before deſcribed may be made to anſwer this purpoſe, I am almoſt afraid to propoſe it. I mean, that within the ſcrew DE another ſtill ſmaller ſhould be made to play, and be connected with the ſcrew CD, ſo as to move round along with it. It muſt have a^2+a+1 threads in an inch, and they muſt be in the contrary direction to thoſe of CD, ſo that when they are both turned together, and CD moves upwards, this other one may move downwards. At one turn of the handle this will move upwards $\frac{1}{a^2+a} \times \frac{1}{a^2+a+1} = \frac{1}{a^4+2a^3+2a^2+a}$ of an inch, and at the ſame time will move round in a circular direction. For example, let CD have 5 threads $(=a)$ in an inch, DE 6 $(=a1)$, and a third ſcrew within DE, but connected with CD ſo as to partake of its motion, 31 $(=a^2+a+1)$. At one turn of the handle, this ſcrew will riſe upwards $\frac{1}{5} \times \frac{1}{6} \times \frac{1}{31} = \frac{1}{930}$ of an inch; but this appears too complicated for uſe, and the leaſt inaccuracy in the conſtruction would hinder it from moving.

But, on the other hand, if while the point E riſes it is of conſequence that it be kept from going round, the machine under conſideration will beſt anſwer this purpoſe. On this principle it may be uſeful in ſeveral reſpects: fot inſtance, let A (fig. 2.) repreſent a magnifying lens, and let it be moveable upon the ſcrew BC of 16 threads in an inch, which turns within the larger ſcrew CD of 15 threads in an inch, and that again moves within the plate EF in the end of the cylinder GF*. To uſe the inſtrument, fix the object to be magnified upon the pin GL, and then turn the lens A upon the ſcrew BC, till it be nearly at the proper diſtance from the pin, and oppoſite to it. You may then adjuſt the diſtance more accurately by turning the ſcrew DC, at each turn of which the lens will recede from, or approach to, the pin $\frac{1}{142}$th of an inch. This it will do and not turn aſide, but ſtill remain oppoſite to the pin LG. A double microſcope might be fitted on in the place of the lens A. The whole inſtrument may be furniſhed with a handle, as at M; or, if larger, it may have three feet to ſtand on a table.

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* The ſcrew BC is reſtrained from moving along with CD by the ſmall pillar HK, which ſlides backwards and forwards in a groove in the cylinder GF.

On the laſt principle it muſt be owned, the common ſcrew has the advantage, as two ſcrews will produce more friction than one; and, beſides, in the compound engine there is an additional friction from the piece FG (fig. 1.) upon the pillars between which it moves.

Another caſe in which this machine may be employed is in the *micrometer*. Thus, let the ſcrew AB (fig. 3) of 50 threads in an inch be turned round by the index C, which moves upon the graduated circle ECD in the direction CD. Within the ſcrew AB is the ſmaller one AF of 51 threads in an inch, retained from moving round by the bar GFH. The piece AF is continued to K, where it forms a fine point. To uſe the inſtrument, let it be adjuſted to the teleſcope or microſcope by which you are to view a ſtar, or ſome ſmall object, and let the point K appear juſt to touch one edge of the object. Then turn the index C, and the point K will advance upwards till it appears to cover the other edge of the object, and thus you can determine its ſize. The point K will advance at each complete turn of the index $\frac{1}{2550}$ of an inch; and if the circle be divided into 80 equal parts, one of which, if it is an inch in diameter, will be very obſervable, while the index moves over one of theſe, the point K will advance $\frac{1}{204000}$ of an inch.

Thus, for example, ſuppoſe I am to meaſure the diameter of a nervous fibre in the medullary ſubſtance of the brain, I make the point K appear cloſe to one edge, and turn the index till the ſame point paſs over the fibre, and appear to touch the other edge: I then look on the graduated circle ECD, and perceive that the index C has paſſed over, ſuppoſe, 23,2 diviſions. Hence I conclude the diameter of the fibre to be $23,2 \times \frac{1}{204000} = \frac{1}{8790}$ of an inch, which is nearly the ſize as found by the accurate obſervations of Dr. MONRO. There ſhould be a NONIUS's ſcale on the index which will meaſure to one tenth of a diviſion.

As the index C muſt continue cloſe to the plate ECD, while at the ſame time it turns round the ſcrew AB, which is continually riſing, it muſt be made as in fig. 4. where *a*, *b*, are two ſmall pieces which play in a groove in the ſcrew AB (fig. 3.) while the groove CD (fig. 4.) in the index is filled up by a protuberance of the plate ECD (fig. 3.); the piece below the groove *cd* (fig. 4.) being ſunk into that plate. The whole machinery may be incloſed in a cylinder of braſs reaching from B to L (fig. 3.), ſo that the point of the ſcrew KL may be without it, and the ſides of the cylinder may be open at ECD.

It is farther to be obſerved, that what has been ſaid goes on the ſuppoſition that the point K, in the micrometer, is equally magnified with the object we are to meaſure. But, if this point be placed in the focus of the eye-glaſs of a double microſcope; when it moves it will paſs over, not the object itſelf, but its image, magnified by the object-glaſs. In this caſe, if the object-glaſs magnify the diameter 10 times, while the index paſſes over one diviſion, the point K will paſs over the image of an object, the diameter of which is $\frac{1}{2040000}$ of an inch. As in this mode of application the point K muſt fall between the object and eye-glaſs, the ſcrews may be contained within the *fulcrum* by which the microſcope is ſupported.

The machine (fig. 1.) may be applied as a jack to raiſe great weights a little way from the ground, by ſubſtituting two croſs hand-ſpikes for the handle CKL; or a vertical handle may be employed in the following manner. Let A (fig. 5.) be a pinion turned by the handle AB, which we ſuppoſe a foot in length. Let the pinion A have 4 teeth, and move the wheel CD of 16 teeth. The ſcrew EF of 4 threads in an inch is fixed in this wheel, and turns round along with it. Within it plays the ſcrew FG of 5 threads in an inch, and which we ſuppoſe prevented from following the motion of EF: it terminates in ſuch a ſhoulder as that repreſented at G, and being continued to H ends in a foot as in the figure. The whole is incloſed in a ſtrong frame. The pinion A muſt be connected in ſuch a manner with the wheel CD as to riſe within the frame along with it, which may eaſily be done by making its axis play in a piece of wood or metal, which is connected by the end to the ſcrew EF. Or, if this ſhould be deemed inconvenient, as the riſing of the pinion muſt raiſe the handle AB, the wheel CD may be hindered from riſing, and at the ſame time turn the ſcrew EF, by a contrivance ſimilar to that uſed with the index C (fig. 3.) in the micrometer. In either caſe, the axis of the pinion ſhould be continued through the oppoſite ſide of the frame, and armed with a heavy fly to regulate the motion. When the machine is to be applied to uſe, the bottom of the frame reſting on the ground, if the body to be lifted is already as high as the top G, that top is applied below it; but if it is cloſe to the ground, we put below it the foot H; then, if the handle AB be turned once round, the wheel CD and ſcrew EF will turn $\frac{1}{4}$ part round, and the point F will riſe ($\frac{1}{4} \times \frac{1}{4} =$) $\frac{1}{16}$th of an inch. The point G or H will therefore be lifted upwards ($\frac{1}{16} \times \frac{1}{5} =$) $\frac{1}{80}$th of an inch. But the end B of the handle AB has deſcribed above ſix feet; therefore the velocity of the point G is to that of the point B as one to ($72 \times 80 =$) 5760. Therefore, if we ſuppoſe a man to act at the handle with a force equal to 30 lbs. he may keep in equilibrio a weight of 172800 lbs. But a ſubduction of perhaps more than one half of this muſt be made, that he may raiſe the weight, as the friction of the engine will be conſiderable. Suppoſe it to be two-thirds, the effect ſtill remains equal to 57600 lbs. or 25 tons 14 cwts. and 32 lbs.

It will eaſily appear, that this method of applying the ſcrew may have a place in many other engines, particularly where great accuracy is required; or we want a motion to be performed with great power, while at the ſame time it need not have any large compaſs. The few examples given above may ſerve as a ſpecimen.

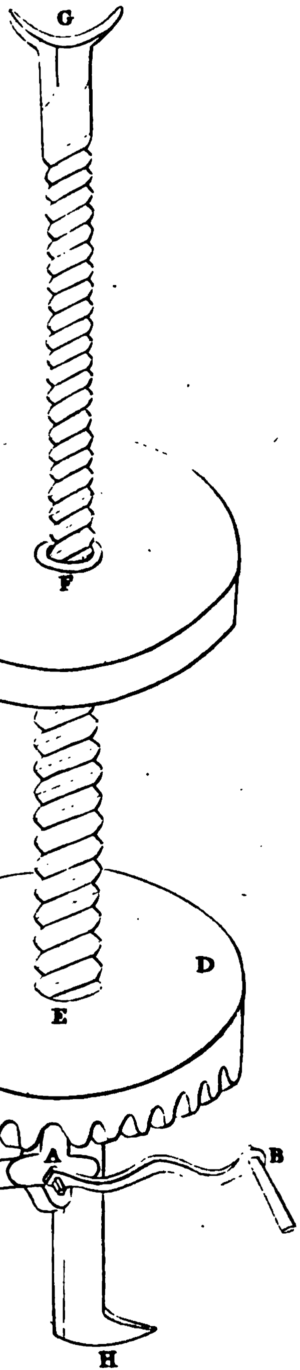
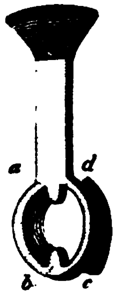


Fig. 5.

Fig. 4.



VI. *An Account of the* Turkey. *By* Thomas Pennant, *Eſq.* *F. R. S.*; *communicated by* Joſeph Banks, *Eſq. P. R. S.*

Read December 21, 1781.

TURKEY. BILL convex, ſhort and ſtrong.

HEAD and neck covered with a naked tuberoſe fleſh, with a long fleſhy appendage hanging from the baſe of the upper mandible.

ON the breaſt a long tuft of coarſe black hairs.

Wild Turkey. JOSSELYN's Voy. 99. Rarities 8. CLAYTON's Virgin: Phil. Tranſ. abridged, III. 590. LAWSON, 149. CATESBY Topp. XLIV.

Le coq d'Inde, BELON 248.

Gallo-pavo, GESNER Av. 481. Icon. 56.

Gallo-pavo, ALDROV. Av. II. 18.

Gallo-pavo, the Turkey, A. 3. Gallo-pavo ſylveſtris Novæ Angliæ, a New England wild Turkey, RAII Synopſis Avium 51.

Meleagris Gallo-pavo. M. capite caruncula frontali gularique, maris pectore barbato, LIN. Syſt. 268.

Le Dindon de BUFFON III. BRISSON. I. 158. tab. XVI. Pl. Enl. 97.

Deſcription.

Deſcription. T. with the characters deſcribed in the definition of the genus. The plumage, dark gloſſed with variable copper colour, and green. Coverts of the wings and the quill feathers barred with black and white. Tail. Tail conſiſts of two orders. The upper or ſhorter very elegant, the ground colour a bright bay; the middle feather marked with numerous bars of ſhining black and green. The greateſt part of the exterior feathers of the ſame ground with the others marked with three broad bands of mallard green, placed remote from each other. The two next are coloured like thoſe of the middle; but the end is plain and croſſed with a ſingle bar, like the exterior.

The longer or lower order are of a ruſty white colour, mottled with black; and croſſed with numerous narrow-waved lines of the ſame colour; and near the end with a broad band.

Wild Turkies preſerve a ſameneſs of colouring; the tame, as uſual with domeſtic animals, vary. It is needleſs to point out the differences in ſo well known a bird: the black approaches neareſt to the original ſtock. This variety I have ſeen nearly in a ſtate of nature in Richmond and other parks. A moſt beautiful kind has of late been introduced into England of a ſnowy whiteneſs, finely contraſting with its red head. White variety. Theſe, I think, came out of Holland, probably bred from an accidental white pair; and from them preſerved pure from any dark or variegated birds.

Size. The ſizes of the wild Turkies have been differently repreſented. Some writers aſſert, that there have been inſtances of their weighing ſixty pounds; but I find none who, ſpeaking from their own knowlege, can prove their weight to be above forty. JOSSELYN ſays, that he has eaten part of a cock, which after it was plucked, and the entrails taken out, weighed thirty *. LAWSON, whoſe authority is unqueſtionable, ſaw half a Turkey ſerve eight hungry men for two meals †; and ſays, that he had ſeen others which he believed weighed forty pounds. CATESBY tells us, that out of the many hundreds which he had handled §, very few exceeded thirty pounds; each of theſe ſpeak of their being double that ſize merely from the reports of others.

Manners. The manners of theſe birds are as ſingular as their figure. Their attitudes in the ſeaſon of courtſhip are very ſtriking. The males fling their heads and neck backwards, briſtle up their feathers, drop their wings to the ground, ſtrut and pace moſt ridiculouſly; wheel round the females with their wings ruſtling along the earth, at the ſame time emitting a ſtrange ſound through their noſtrils
Notes. not unlike the *Grurr* of a great ſpinning wheel. On being interrupted they fly into great rages, and change their notes into a loud and guttural gobble,

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* New England Rarities, p. 8.

† Hiſtory of Carolina, p. 149. and 27.

§ App. XLIV. The greateſt certain weight is given by Mr. CLAYTON, who ſaw one that reached 38 lbs. Phil. Tranſ.

and then return to dalliance. The ſound of the female is plaintive and melancholy.

Iraſcible. The paſſions of the males are very ſtrongly expreſſed by the change of colours in the fleſhy ſubſtance of the head and neck, which alters to red, white, blue, and yellowiſh, as they happen to be affected. The ſight of any thing red excites their choler greatly.

Polygamous. They are polygamous, one cock ſerving many hens. They lay in the ſpring, and produce a great number of eggs. They will perſiſt in laying for a great while. They retire to ſome obſcure place to ſit, the cock through rage at the loſs of its mate being very apt to break the eggs. The females are very affectionate to their young, and make great moan on the loſs of them. They ſit on their eggs with ſuch perſeverance, that if they are not taken away when addle, the hens will almoſt periſh with hunger before they will quit the neſt.

Turkies greatly delight in the ſeeds of nettles; but thoſe of the purple-fox glove prove fatal to them [*].

Turkies are very ſtupid birds, quarrelſome, and cowardly. It is diverting to ſee a whole flock attack the common cock, who will, for a long time, keep a great number at bay.

Swift. Perch high. They are very ſwift runners in the tame as well as the wild ſtate: they are but indifferent flyers. They love to perch on trees, and gain the height they wiſh by riſing from bough to bough. In a wild ſtate they get to the very ſummit of the loftieſt trees, even ſo high as to be beyond the reach of the muſquet [*].

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[*] DE BUFFON.

Gregarious. In the ſtate of nature they go in flocks even of five hundred [†], feed much on the ſmall red acorns, and grow ſo fat in *March* that they cannot fly more than three or four hundred yards, and are ſoon ran down by a horſeman. In the unfrequented parts bordering on the *Miſſiſipi*, they are ſo tame as to be ſhot with even a piſtol [‡].

Haunts. They frequent the great ſwamps [§] of their native country, and leave them at ſun-riſing to repair to the dry woods in ſearch of acorns and berries; and before ſun-ſet retire to the ſwamps to rooſt.

The fleſh of the wild Turkey is ſaid to be ſuperior in goodneſs to the tame, but redder. Eggs of the former have been taken from the neſt, and hatched under tame Turkies. The young will ſtill prove wild, perch ſeparate, yet mix and breed together in the ſeaſon. The Indians ſometimes uſe the breed produced from the wild as decoy birds to ſeduce thoſe in a ſtate of nature within their reach [||].

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[*] LAWSON, 45.

[†] ADAIR's Amer. 360.

[‡] LAWSON, 149.

[§] It is in the ſwamps that the loftieſt and moſt bulky trees are. The wet with which they are environed makes them a ſecure retreat.

[||] LAWSON, 149.

Wild Turkies are now grown moſt exceſſively rare in the inhabited parts of *America*, and are only found in numbers in the diſtant and moſt unfrequented ſpots.

The Indians make a moſt elegant cloathing of the feathers. They twiſt the inner webs into a ſtrong double thread of hemp, or inner bark of the mulberry tree, and work it like matting; it appears very rich and gloſſy, and as fine as a ſilk ſhag[*]. They alſo make fans of the tail; and the French of *Louiſiana* were wont to make umbrellas by the junction of four of the tails[†].

When diſturbed, they do not take wing, but run out of ſight. It is uſual to chaſe them with dogs, when they will fly and perch on the next tree. They are ſo ſtupid or ſo inſenſible of danger, as not to fly on being ſhot at; but the ſurvivors remain unmoved at the death of their companions[‡].

Place.

Turkies are natives only of *America*, or the New World, and of courſe unknown to the ancients. Since both theſe poſitions have been denied by ſome of the moſt eminent naturaliſts of the ſixteenth century, I beg leave to lay open, in as few words as poſſible, the cauſe of their error.

Miſtaken by BELON.

BELON[§], the earlieſt of thoſe writers who are of opinion that theſe birds were natives of the old world, founds his notion on the deſcription of the Guinea fowl, the Meleagrides of STRABO, ATHENÆUS, PLINY, and others of the ancients. I reſt the refutation on the excellent account given by ATHENÆUS, taken from CLYTUS MILESIUS, a diſciple of ARISTOTLE, which can ſuit no other than that fowl. "They want," ſays he, "natural affection towards their young; their head is naked, and on the top is a hard round body like a peg or nail: from their cheeks hangs a red piece of fleſh like a beard. It has no wattles like the common poultry. The feathers are black, ſpotted with white. They have no ſpurs; and both ſexes are ſo like as not to be diſtinguiſhed by the ſight." VARRO [*] and PLINY [†] take notice of the ſpotted plumage and the gibbous ſubſtance on the head. ATHENÆUS is more minute, and contradicts every character of the Turkey, whoſe females are remarkable for their natural affection, and differ materially in form from the males, whoſe heads are deſtitute of the callous ſubſtance and whoſe heels (in the males) are armed with ſpurs.

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[*] LAWSON, 18. ADAIR, 423.

[†] DU PRATZ, II. 85.

[‡] DU PRATZ, 224.

[§] Hiſt. des Oys. 248.

ALDROVANDUS;

ALDROVANDUS, who died in 1605, draws his arguments from the ſame ſource as BELON; I therefore paſs him by, and take notice of the greateſt of our naturaliſts GESNER [‡], who falls into a miſtake of another kind, and wiſhes the

and GESNER.

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[*] Lib. III. c. 9.

[†] Lib. X. c. 26.

[‡] Av. 481.

Turkey to be thought a native of India. He quotes ÆLIAN for that purpoſe, who tells us, "That in India are very large poultry not with "combs, but with various coloured creſts inter"woven like flowers, with broad tails neither "bending nor diſplayed in a circular form, which "they draw along the ground as peacocks do "when they do not erect them; and that the "feathers are partly of a gold colour, partly "blue, and of an emerald colour"[*].

This in all probability was the ſame bird with the Peacock Pheaſant of Mr. EDWARDS, *Le Paon de Tibet* of M. BRISSON, and the *Pavo bicalcaratus* of LINNÆUS. I have ſeen this bird living. It has a creſt, but not ſo conſpicuous as that deſcribed by ÆLIAN; but it has thoſe ſtriking colours in form of eyes, neither does it erect its tail like the Peacock[†], but trails it like the Pheaſant. The *Catreus* of STRABO[‡] ſeems to be the ſame bird. He deſcribes it as uncommonly beautiful and ſpotted, and very like a Peacock. The former author[§] gives a more minute account of this ſpecies, and under the ſame name. He borrows it from CLITARCHUS, an attendant of ALEXANDER the Great in all his conqueſts. It is evident from his deſcription, that it was of this kind; and it is likewiſe probable, that it was the ſame with his large Indian poultry before cited. He celebrates it alſo for its fine note; but allowance muſt be made for the credulity of ÆLIAN. The *Catreus*, or Peacock Pheaſant, is a native of Tibet, and in all probability of the north of India, where CLITARCHUS might have obſerved it; for the march of ALEXANDER was through that part which borders on Tibet, and is now known by the name of *Penj-ab* or five rivers.

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[*] De Anim. lib. XVI. c. 2.

[†] Edw. II. 67.

[‡] Lib. XV. p. 1046

[§] De Anim. lib. XVII. c. 23.

Not natives of Europe;

I ſhall now collect from authors the ſeveral parts of the world where Turkies are unknown in the ſtate of nature. Europe has no ſhare in the queſtion; it being generally agreed that they are exotic in reſpect to that continent.

nor of Aſia;

Neither are they found in any part of Aſia Minor, or the Aſiatic Turky, notwithſtanding ignorance of their true origin firſt cauſed them to be named from that empire. About Aleppo, capital of Syria, they are only met with, domeſticated like other poultry *. In Armenia they are unknown, as well as in Perſia; having been brought from Venice by ſome Armenian merchants into that empire †, where they are ſtill ſo ſcarce as to be preſerved among other rare fowl in the royal menagery ‡.

DU HALDE acquaints us, that they are not natives of China; but were introduced there from other countries.

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* RUSSEL, 63.

† TAVERNIER, 146.

‡ BELL's Travels, I. 128.

countries. He errs from miſinformation in ſaying that they are common in India.

I will not quote GEMELLI CARERI, to prove that they are not found in the Philippine Iſlands, becauſe that gentleman with his pen travelled round the world in his eaſy chair, during a very long indiſpoſition and confinement [*] in his native country.

But DAMPIER bears witneſs that none are found in Mindanao [‡].

nor Africa;

The hot climate of Africa barely ſuffers theſe birds to exiſt in that vaſt continent, except under the care of mankind. Very few are found in Guinea, except in the hands of the Europeans, the negroes declining to breed any on account of the great heats [§]. PROSPER ALPINUS ſatisfies us, they are not found either in Nubia or in Egypt. He deſcribes the Meleagrides of the ancients, and only proves that the Guinea hens were brought out of Nubia, and ſold at a great price at Cairo [‖]; but is totally ſilent about the Turkey of the moderns.

Let me in this place obſerve, that the Guinea hens have long been imported into Britain. They were cultivated in our farm-yards; for I diſcover in 1277, in the Graingc of Clifton, in the pariſh of Ambroſden in Buckinghamſhire, among other articles, ſix *Mutilones* and ſix *Africanæ fœminæ* [*], for this fowl was familiarly known by the names of Afra Avis and Gallina Africana and Numida. It was introduced into Italy from Africa, and from Rome into our country. They were neglected here by reaſon of their tenderneſs and difficulty of rearing. We do not find them in the bills of fare of our ancient feaſts [†]; neither do we find the Turkey: which laſt argument amounts to almoſt a certainty, that ſuch a hardy and princely bird had not found its way to us. The other likewiſe was then known by its claſſical name; for that judicious writer Doctor CAIUS deſcribes, in the beginning of the reign of ELIZABETH, the Guinea fowl, for the benefit of his friend GESNER, under the name of Meleagris, beſtowed on it by ARISTOTLE [‡].

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[*] Sir JAMES PORTER's Obſ. Turkey, I. 1. 321.

[‡] BARBOT in CHURCHILL's Coll. V. 29.

[§] BOSMAN, 229.

[‖] Hiſt. Nat. Ægypti. I. 201.

Having denied, on the very beſt authorities, that the Turkey ever exiſted as a native of the old world, I muſt now bring my proofs of its being only a native of the new, and of the period in which it firſt made its appearance in Europe.

but of America,

The firſt preciſe deſcription of theſe birds is given by OVIEDO, who in 1525 drew up a ſummary of his greater work, the Hiſtory of the

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[*] KENNET's Parochial Antiq. 287.

[†] Neither in that of GEORGE NEVIL nor among the delicacies mentioned in the Northumberland houſhold book begun in the beginning of the reign of HENRY VIII.

[‡] CAII Opuſc. 13. Hiſt. An. lib. VI, c. 2.

Indies, for the uſe of his monarch CHARLES V. the iſlands, This learned man had viſited the Weſt Indies and its iſlands in perſon, and payed particular regard to the natural hiſtory. It appears from him, that the Turkey was in his days an inhabitant of the greater iſlands, and of the main-land. He ſpeaks of them as Peacocks; for being a new bird to him, he adopts that name from the reſemblance he thought they bore to the former. "But," ſays he, "the neck is bare of feathers, "but covered with a ſkin which they change "after their phantaſie into diverſe colours. They "have * a horn as it were on their front, and "haires on the breaſt †." He deſcribes other birds which he alſo calls Peacocks. They are of the gallinaceous genus, and known by the name of Curaſſao birds, the male of which is black, the female ferruginous.

The next who ſpeaks of them as natives of the main-land of the warmer parts of America, is FRANCISCO FERNANDEZ, ſent there by PHILIP II. to whom he was phyſician. This naturaliſt Mexico, obſerved them in Mexico. We find by him, that the Indian name of the male was Huexolotl, of the female Cihuatotolin. He gives them the title of Gallus Indicus and Gallo Pavo. The Indians, as well as Spaniards, domeſticated theſe uſeful birds. He ſpeaks of the ſize by compariſon, ſaying, that the wild were twice the magnitude of

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* In the Spaniſh Peçon corto.

† In PURCHAS, III. 995.

the tame; and that they were ſhot with arrows or guns[*]. I cannot learn the time when **FERNANDEZ** wrote. It muſt be between the years 1555 and 1598, the period of **PHILIP**'s reign.

Darien. **PEDRO DE CIESA** mentions Turkies on the Iſthmus of Darien[†]. **LERY**, a Portugueſe author, aſſerts, that they are found in Brazil, and gives them an Indian name[‡]; but ſince I can diſcover no traces of them in that diligent and excellent naturaliſt **MARCGRAVE**, who reſided long in that country, I muſt deny my aſſent. But the former is confirmed by that able and honeſt navigator **DAMPIER**, who ſaw them frequently, as well wild as tame, in the province of Yucatan[§], now reckoned part of the kingdom of Mexico. Yucatan.

North America. In North America they were obſerved by the very firſt diſcoverers. When **RENÉ DE LAUDONNIERE**, patronized by Admiral **COLIGNI**, attempted to form a ſettlement near the place where Charleſtown now ſtands, he met with them on his firſt landing in 1564, and by his hiſtorian has repreſented them with great fidelity in the fifth plate of the recital of his voyage[||]: from his time the witneſſes to their being natives of the continent are innumerable. They have been ſeen in flocks of hundreds in all parts from Louiſiana even to Canada; but at this time are extremely rare in a wild ſtate, except in the more diſtant parts, where they are ſtill found in vaſt abundance.

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[*] Hiſt. Av. Nov. Hiſp. 27.

[†] Seventeen Years Travels, 20.

[‡] In **DE LAET**'s Deſcr. des Indes, 491.

[§] Voyages, vol. II. part II. p. 65. 85. 114.

[||] **DE BRY.**

When firſt introduced into Europe.

It was from Mexico or Yucatan that they were firſt introduced into Europe; for it is certain, that they were imported into England as early as the year 1524, the 15th of HENRY VIII *.

We probably received them from Spain, with which we had great intercourſe till about that time. They were moſt ſucceſsfully cultivated in our kingdom from that period; inſomuch, that they grew common in every farm-yard, and became even a diſh in our rural feaſts by the year 1585; for we may certainly depend on the word of old TUSSER, in his Account of the Chriſtmas Huſbandlie Fare †.

Beefe, Mutton, and Porke, ſhred pies of the beſt,
Pig, Veale, Gooſe, and Capon, and Turkie well dreſt,
Cheeſe, Apples, and Nuts, jolie carols to heare,
As then in the countrie, is counted good cheare.

But at this very time they were ſo rare in France, that we are told, that the very firſt which was eaten in that kingdom appeared at the nuptial feaſt of CHARLES IX. in 1570 ‡.

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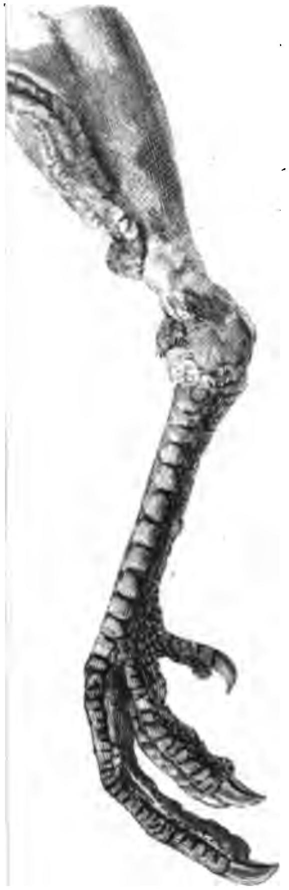
* BAKER'S Chr. ANDERSON'S Dict. Com. I. 354. HACKLUYT, II. 165. makes their introduction about the year 1532. BARNABY GOOGE, one of our early writers on Huſbandry, ſays, they were not ſeen here before 1530. He highly commends a Lady HALES of Kent, for her excellent management of theſe fowl, p. 166.

† Five Hundred Points of good Huſbandrie, p. 57.

‡ ANDERSON'S Dict. Comm. I. 410.



To this account I beg leave to lay before you the very extraordinary appearance on the thigh of a Turkey, bred in my poultry yard, and which was killed a few years ago for the table. The ſervant in plucking it was very unexpectedly wounded in the hand. On examination the cauſe appeared ſo ſingular, that the bird was brought to me. I diſcovered, that from the thigh-bone iſſued a ſhort upright proceſs, and to that grew a large and ſtrong toe, with a ſharp and crooked claw, exactly reſembling that of a rapacious bird.

VII. *Account of a* Nebula *in* Coma Berenices. *By* Edward Pigott, *Eſq. In a Letter to* Nevil Maſkelyne, *D. D. F. R. S. and Aſtronomer Royal.*

Read January 11, 1781.

Frampton-houſe, Glamorganſhire,
September 3, 1779.

REV. SIR,

AS my father generally addreſſes to you ſuch papers as he communicates to the Royal Society, I beg the favour of you to acquaint that learned body, that, on the 23d of March, I diſcovered a nebula in the conſtellation of Coma Berenices, hitherto, I preſume, unnoticed; at leaſt not mentioned in M. DE LA LANDE's Aſtronomy, nor in M. MESSIER's ample Catalogue of nebulous Stars. I have obſerved it in an achromatic tranſit inſtrument, three feet long, and deduced its mean R. A. by comparing it to the following ſtars, having made the neceſſary corrections for aberration and nutation, the reſults are:

	°	′	″
By δ Gemini - - - -	191	28	35
By γ Canis Majoris - - -	191	28	41
By ε Virginis - - - -	191	28	45
By ν Virginis - - - -	191	28	36
By δ Leonis - - - -	191	28	34
Mean R. A. of the nebula for April 20, 1779,	191	28	38

Its light being exceedingly weak, I could not ſee it in the two-feet teleſcope of our quadrant, ſo was obliged to determine its declination likewiſe by the tranſit inſtrument. This determination, however, I believe, may be depended upon to two minutes: hence its declination north is 22° 53″¼. The diameter of this nebula I judged to be about two minutes of a degree.

I am, &c.

E. PIGOTT.

VIII. *Double Stars diſcovered in* 1779, *at* Frampton-houſe, Glamorganſhire. *By* Nathanael Pigott, *Eſq. F. R. S. Foreign Member of the Academies of* Bruſſels *and* Caen, *and Correſpondent of the Royal Academy of Sciences at* Paris; *communicated by* Nevil Maſkelyne, *D. D. F. R. S. and Aſtronomer Royal.*

Read January 11, 1781.

TO THE REV. NEVIL MASKELYNE.

REV. SIR, October 27, 1779.

INCLOSED are the determinations of the places of three double ſtars, which I diſcovered this ſummer; at leaſt, I preſume, they have not been obſerved before, becauſe I do not find them inſerted in Dr. BRADLEY's catalogue, publiſhed in the Nautical Almanac 1773, or in the *Connoiſſance des Tems*, no more than in other catalogues in my poſſeſſion. γ Delphini, indeed, is in M. DE LA CAILLE's catalogue; but not as a double ſtar. The inſtrument he uſed was not, probably, powerful enough for that purpoſe. In the two-feet teleſcope of my quadrant it appears only as a ſingle ſtar. Theſe ſtars were obſerved by me in a three-feet achromatic teleſcope of a tranſit inſtrument, with an object-glaſs near two inches diameter. The R. A. are nicely determined by ſeveral obſervations, which always agree with each to a fraction of a ſecond in time. The declinations were deduced from the difference of altitudes between the double ſtars and the known ſtars, to which they were compared, as ſhewn by the graduated ſemi-circle of the tranſit inſtrument, which, being divided to two minutes only, cannot be ſuppoſed to give any great preciſion: however, I believe, their declinations, hence deduced, to be correct to one minute, or one minute and a half.

In obſerving the double ſtar compared to ζ Pegaſi, I found it impoſſible to illuminate the wires of the tranſit inſtrument, without nearly obliterating the ſtar. This difficulty led me to a method, which, as it completely ſucceeded, may, under ſimilar circumſtances, poſſibly be of uſe to others. A perſon illuminating the wires very faintly, ſo that I could juſt perceive the double ſtar, at a ſignal totally withdrew the light: this ſignal I made when the double ſtar was nearly as far diſtant from the firſt wire as the five wires are diſtant from each other, which, in this caſe, was 13″ by the clock. I then counted the ſeconds, and did not fail to ſee the ſtar diſappear a ſecond or two within the time expected. On its diſappearing, I made a ſignal to write down; and then beginning to count again, did the ſame at each wire. I have ſince tried this method with other ſtars, and think they may be thus obſerved, even with greater preciſion than when the wires are illuminated. Hence the troubleſome buſineſs, well known to aſtronomers, of illuminating faint objects, may be removed.

The preceding ſtar of each double ſtar was obſerved on the firſt, third and fifth wires, and the following one on the ſecond and fourth wires; and thus their difference in R. A. in time, converted into parts of a great circle, obtained. Suppoſing the apparent R. A. and App. declinations of α Delphini, β Aquarii, and ζ Pegaſi, as here aſſumed, the places of theſe double ſtars were found to be as follows.

I beg you to communicate theſe obſervations to the Royal Society; and to receive my thanks for thoſe I received in your letter of the 7th inſtant.

I am, &c.

N. PIGOTT.

September 5, 1779.

App. R. A.					App. declination.			
°	′	″			°	′	″	
307	21	5	α Delphini, 3d mag.	-	15	8	53	N.
309	6	30	2d or brighteſt of γ Delphini 4.		15	20	40	N.
	0	9½	diff. R.A. of the 2 ſtars in γ Delphini.					

Note, both the ſtars in γ Delphini have the ſame, or nearly the ſame, declination. The 1ſt is of the 6th, the 2d of the 4th mag.

September 19.

319	59	27 +	β Aquarii, 3d mag.	-	6	31	34	S.
318	3	21	preceding double ſtar, 5th mag.		7	40	34	S.
	0	11 -	diff. R.A. between 1ſt and 2d of the double ſtar.					

Note, the 1ſt ſeemed of the 5th, the 2d of the 7th mag. The 1ſt is perhaps 6″ or 8″ S. of the following one.

337	36	55 +	ζ Pegaſi, 3d mag.	-	9	41	24	N.
346	53	36½	double ſtar, 8th, 9th mag	-	3	59	17	N.

Note, both the ſtars of this double ſtar have the ſame, or nearly the ſame, R. A.; their difference in declination is 15″ or perhaps 20″.

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IX. *An Account of the* Ganges *and* Burrampooter *Rivers. By* James Rennell, *Eſq. F. R. S.; communicated by* Joſeph Banks, *Eſq. P. R. S.*

Read January 25, 1781.

THE * Ganges and † Burrampooter Rivers, together with their numerous branches and adjuncts, interſect the country of Bengal in ſuch a variety of directions, as to form the moſt compleat and eaſy inland navigation that can be conceived. So equally and admirably diffuſed are thoſe natural canals, over a country that approaches nearly to a perfect plane, that, after excepting the lands contiguous to Burdwan, Birboom, &c. (which altogether do not conſtitute a ſixth part of Bengal) we may fairly pronounce, that every other part of the country has, even in the dry ſeaſon, ſome navigable ſtream within 25 miles at fartheſt, and more commonly within a third part of that diſtance.

It is ſuppoſed, that this inland navigation gives conſtant employment to 30,000 boatmen. Nor will it be wondered at, when it is known, that all the ſalt, and a large proportion of the food conſumed by ten millions of people are conveyed by water within the kingdom of Bengal and its dependencies. To theſe muſt be added, the tranſport of the commercial exports and imports, probably to the amount of two millions ſterling *per annum*; the interchange of manufactures and products throughout the whole country; the fiſheries; and the article of travelling *.

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* The proper name of this river in the language of Hindooſtan (or Indoſtan) is Pudda or Padda. It is alſo named Burra-Gonga, or the Great River; and Gonga, the River, by way of eminence; and from this, doubtleſs, the European names of the river are derived.

† The orthography of this word, as given here, is according to the common pronunciation in Bengal; but it is ſaid to be written in the Sanſcrit language, Brahma-pootar, which ſignifies the Son of Brahma.

Theſe rivers, which a late ingenious gentleman aptly termed ſiſters and rivals (he might have ſaid *twin* ſiſters, from the contiguity of their ſprings), exactly reſemble each other in length of courſe; in bulk, until they approach the ſea; in the ſmoothneſs and colour of their waters; in the appearance of their borders and iſlands; and, finally, in the height to which their floods riſe with the periodical rains. Of the two, the Burrampooter is the largeſt; but the difference is not obvious to the eye. They are now well known to derive their ſources from the vaſt mountains of Thibet †; from whence they proceed in oppoſite directions; the Ganges ſeeking the plains of Hindooſtan (or Indoſtan) by the weſt; and the Burrampooter by the eaſt; both purſuing the early part of their courſe through rugged vallies and defiles, and ſeldom viſiting the habitations of men. The Ganges, after wandering about 750 miles through theſe mountainous regions, iſſues forth a deity to the ſuperſtitious, yet gladdened, inhabitant of Hindooſtan[*]. From Hurdwar (or Hurdoar) in latitude 30°, where it guſhes through an opening in the mountains, it flows with a ſmooth navigable ſtream through delightful plains during the remainder of its courſe to the ſea (which is about 1350 miles) diffuſing plenty immediately by means of its living productions; and ſecondarily by enriching the adjacent lands, and affording an eaſy means of tranſport for the productions of its borders. In a military view, it opens a communication between the different poſts, and ſerves in the capacity of a *military way* through the country; renders unneceſſary the forming of magazines; and infinitely ſurpaſſes the celebrated inland navigation of North America, where the *carrying places* not only obſtruct the progreſs of an army, but enable the adverſary to determine his place and mode of attack with certainty.

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* The embarkations made uſe of vary in bulk from 180 tons down to the ſize of a wherry. Thoſe from 30 to 50 tons are reckoned the moſt eligible for tranſporting merchandize.

† Theſe are amongſt the higheſt of the mountains of the old hemiſphere. I was not able to determine their height; but it may in ſome meaſure be gueſſed, by the circumſtance of their riſing conſiderably above the horizon, when viewed from the plains of Bengal, at the diſtance of 150 miles.

In its courſe through the plains, it receives eleven rivers, ſome of which are equal to the Rhine, and none ſmaller than the

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[*] The fabulous account of the origin of the Ganges (as communicated by my learned and ingenious friend C. W. BOUGHTON ROUSE, Eſq.) is, that it flows out of the foot of Beſchan[*] (from whence, ſay the Bramins, it has its name Padda; that word ſignifying foot in the Sanſcrit language); and that in its courſe to the plains of Hindooſtan it paſſes through an immenſe rock ſhaped like a Cow's-head.

The allegory is highly expreſſive of the veneration which the Hindoos have for this famous ſtream; and no leſs ſo of their gratitude to the Author of Nature for beſtowing it: for it deſcribes the bleſſing as flowing purely from his bounty and goodneſs.

The rock before mentioned has, I believe, never been viſited by any European; and is even allowed by moſt of the natives to bear no reſemblance to the object from whence it is denominated. However, as the effects of ſuperſtition do often long ſurvive the illuſions that gave it birth, the rock or cavern ſtill preſerves the name of *Gowmooky*, or Cow's-head.

[*] Beſchan is the ſame with Viſtnou, the PRESERVING DEITY.

Thames, beſides as many others of leſſer note. It is owing to this vaſt influx of ſtreams, that the Ganges exceeds the Nile ſo greatly in point of magnitude, whilſt the latter exceeds it in length of courſe by one-third. Indeed, the Ganges is inferior in this laſt reſpect, to many of the northern rivers of Aſia; though I am inclined to think that it diſcharges as much or more water than any of them, becauſe thoſe rivers do not lie within the limits of the periodical rains *.

The bed of the Ganges is, as may be ſuppoſed, very unequal in point of width. From its firſt arrival in the plains at Hurd-

war,

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* The proportional lengths of courſe of ſome of the moſt noted rivers in the world are ſhewn nearly by the following numbers:

European Rivers.	
Thames, - -	1
Rhine, - -	5¼
Danube, - -	7
Wolga, - -	9½
Aſiatic rivers.	
Indus, - - -	5½
Euphrates, - -	8⅓
Ganges, - -	9½
Burrampooter, - -	9½
Nou Kian, or Ava River, -	9½
Jenniſea, - -	10
Oby, - -	10½
Amoor, - -	11
Lena, - -	11½
Hoanho (of China), -	13½
Kian Keu (of ditto), -	15½
African river.	
Nile, - -	12½
American rivers.	
Miſſiſipi, - -	8
Amazons, - -	15¾

war, to the conflux of the Jumnah (the firſt river of note that joins it) its bed is generally from a mile to a mile and a quarter wide; and, compared with the latter part of its courſe, tolerably ſtraight. From hence, downward, its courſe becomes more winding, and its bed conſequently wider *, till, having alternately received the waters of the Gogra, Soane, and Gunduck, beſides many ſmaller ſtreams, its bed has attained its full width; although, during the remaining 600 miles of its courſe it receives many other principal ſtreams. Within this ſpace it is, in the narroweſt parts of its bed, half a mile wide, and in the wideſt, three miles; and that, in places where no iſlands intervene. The ſtream within this bed is always either increaſing or decreaſing, according to the ſeaſon. When at its loweſt (which happens in April) the principal channel varies from 400 yards to a mile and a quarter; but is commonly about three quarters of a mile.

The Ganges is fordable in ſome places above the conflux of the Jumnah, but the navigation is never interrupted. Below that, the channel is of conſiderable depth, for the additional ſtreams bring a greater acceſſion of depth than width. At 500 miles from the ſea, the channel is thirty feet deep when the river is at its loweſt; and it continues at leaſt this depth to the ſea, where the ſudden expanſion of the ſtream deprives it of the force neceſſary to ſweep away the bars of ſand and mud thrown acroſs it by the ſtrong ſoutherly winds; ſo that the principal branch of the Ganges cannot be entered by large veſſels.

About 220 miles from the ſea (but 300 reckoning the windings of the river) commences the head of the Delta of the Ganges, which is conſiderably more than twice the area of that of the Nile. The two weſternmoſt branches, named the

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* This will be explained when the windings of the river are treated of.

Coſſimbuzar and Jellinghy Rivers, unite and form what is afterwards named the Hoogly River, which is the port of Calcutta, and the only branch of the Ganges that is commonly navigated by ſhips*. The Coſſimbuzar River is almoſt dry from October to May; and the Jellinghy River (although a ſtream runs in it the whole year) is in ſome years unnavigable during two or three of the dryeſt months; ſo that the only ſubordinate branch of the Ganges, that is at all times navigable, is the Chundnah River, which ſeparates at Moddapour, and terminates in the Hooringotta.

That part of the Delta bordering on the ſea, is compoſed of a labyrinth of rivers and creeks, all of which are ſalt, except thoſe that immediately communicate with the principal arm of the Ganges. This tract, known by the name of the Woods, or Sunderbunds, is in extent equal to the principality of Wales; and is ſo completely enveloped in woods, and infeſted with Tygers, that if any attempts have ever been made to clear it (as is reported) they have hitherto miſcarried. Its numerous canals are ſo diſpoſed as to form a compleat inland navigation throughout and acroſs the lower part of the Delta, without either the delay of going round the head of it, or the hazard of putting to ſea. Here ſalt, in quantities equal to the whole conſumption of Bengal and its dependencies, is made and tranſported with equal facility: and here alſo is found an inexhauſtible ſtore of timber for boat-building. The breadth of the lower part of this Delta is upwards of 180 miles; to which, if we add that of the two branches of the river that bound it, we ſhall have about 200 miles for the diſtance to which the Ganges expands its branches at its junction with the ſea.

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* The Hoogly River, or weſternmoſt branch of the Ganges, has a much deeper outlet to the ſea than the principal branch. Probably this may be owing to its precipitating a leſs quantity of mud than the other; the quantity of the Ganges water diſcharged here being leſs than in the other in the proportion of one to ſix. From the difficulties that occur in navigating the entrance of the Hoogly River, many are led to ſuppoſe, that the channels are ſhallow. The difficulties, however, ariſe from bringing the ſhips acroſs ſome of the ſand-banks, which project ſo far into the ſea, that the channels between them cannot eaſily be traced from without.

It has been obſerved before, that the courſe of this river, from Hurdwar to the ſea, is through an uniform plain, or, at leaſt, what appears ſuch to the eye: for, the declivity is much too ſmall to be perceptible. A ſection of the ground, parallel to one of its branches, in length 60 miles, was taken by order of Mr. HASTINGS; and it was found to have about nine inches deſcent in each mile, reckoning in a ſtraight line, and allowance being made for the curvature of the earth. But the windings of the river were ſo great, as to reduce the declivity on which the water ran, to leſs than four inches *per* mile: and by a compariſon of the velocity of the ſtream at the place of experiment with that in other places, I have no reaſon to ſuppoſe, that its general deſcent exceeds it [*].

The medium rate of motion of the Ganges is leſs than three miles an hour in the dry months. In the wet ſeaſon, and during the draining off of the waters from the inundated lands, the current runs from five to ſix miles an hour; but there are inſtances of its running ſeven, and even eight miles, in particular ſituations, and under certain circumſtances. I have an experiment of my own on record, in which my boat was carried 56 miles in eight hours; and that againſt ſo ſtrong a wind, that the boat had evidently no progreſſive motion through the water.

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[*] M. DE CONDAMINE found the deſcent of the river Amazons, in a ſtraight courſe of about 1860 miles, to be about 1020 Engliſh feet, or $6\frac{1}{3}$ inches in a mile. If we allow for the windings, it comes out nearly the ſame as the Ganges (which winds about $1\frac{1}{3}$ mile in three, taking its whole courſe through the plains), namely, about 4 inches in a mile.

When we conſider, that the velocity of the ſtream is three miles in one ſeaſon, and five or more in the other, on the ſame deſcent of four inches *per* mile; and, that the motion of the inundation is only half a mile *per* hour, on a much greater deſcent; no further proof is required how ſmall the proportion of velocity is, that the deſcent communicates. It is then, to the impetus originating at the ſpring head, or at the place where adventitious waters are poured in, and ſucceſſively communicated to every part of the ſtream, that we are principally to attribute the velocity, which is greater or leſſer, according to the quantity of water poured in.

In common, there is found on one ſide of the river an almoſt perpendicular bank, more or leſs elevated above the ſtream, according to the ſeaſon, and with deep water near it: and on the oppoſite ſide a bank, ſhelving away ſo gradually as to occaſion ſhallow water at ſome diſtance from the margin. This is more particularly the caſe in the moſt winding parts of the river, becauſe the very operation of winding produces the ſteep and ſhelving banks*: for the current is always ſtrongeſt on the external ſide of the curve formed by the ſerpentine courſe of the river; and its continual action on the banks

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* Hence it is, that the ſection of a river, that winds through a looſe ſoil, approaches nearly to an obtuſe angled-triangle, one of whoſe ſides is exceedingly ſhort and diſproportioned to the other two . But when a river perſeveres in a ſtraight courſe, the ſection becomes nearly the half of an ellipſis divided longitudinally .

either undermines them *, or waſhes them down. In places where the current is remarkably rapid, or the ſoil uncommonly looſe, ſuch tracts of land are ſwept away in the courſe of one ſeaſon, as would aſtoniſh thoſe who have not been eye-witneſſes to the magnitude and force of the mighty ſtreams occaſioned by the periodical rains of the tropical regions. This neceſſarily produces a gradual change in the courſe of the river; what is loſt on one ſide being gained on the other, by the mere operation of the ſtream: for the fallen pieces of the bank diſſolve quickly into muddy ſand, which is hurried away by the current along the border of the channel to the point from whence the river turns off to form the next reach, where the ſtream growing weak, it finds a reſting place, and helps to form a ſhelving bank, which commences at the point, and extends downwards, along the ſide of the ſucceeding reach.

To account for the ſlackneſs of the current at the point, it is neceſſary to obſerve, that the ſtrongeſt part of it, inſtead of turning ſhort round the point, preſerves for ſome time the direction given it by the laſt ſteep bank, and is accordingly thrown obliquely acroſs the bed of the river to the bay on the oppoſite ſide, and purſues its courſe along it, till the intervention of another point again obliges it to change ſides.

In thoſe few parts of the river that are ſtraight, the banks undergo the leaſt alteration †, as the current runs parallel to them;

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* In the dry ſeaſon ſome of theſe banks are more than 30 feet high, and often fall down in pieces of many tons weight, and occaſion ſo ſudden and violent an agitation of the water, as ſometimes to ſink large boats that happen to be near the ſhore.

† It is more than probable, that the ſtraight parts owe their exiſtence to the tenacity of the ſoil of which their banks are compoſed. Whatever the cauſe may be, the effect very clearly points out ſuch ſituations as the propereſt for placing towns in.

them; but the leaſt inflection of courſe has the effect of throwing the current againſt the bank; and if this happens in a part where the ſoil is compoſed of looſe ſand, it produces in time a ſerpentine winding.

It is evident, that the repeated additions made to the ſhelving bank before mentioned, become in time an encroachment on the channel of the river; and this is again counter-balanced by the depredations made on the oppoſite ſteep bank, the fragments of which, either bring about a repetition of the circumſtances above recited, or form a bank or ſhallow in the midſt of the channel. Thus a ſteep and a ſhelving bank are alternately formed in the crooked parts of the river (the ſteep one being the *indented* ſide, and the ſhelving one the *projecting*); and thus, a continual fluctuation of courſe is induced in all the winding parts of the river; each meander having a perpetual tendency to deviate more and more from the line of the general courſe of the river, by eating deeper into the bays, and at the ſame time adding to the points, till either the oppoſite bays meet, or the ſtream breaks through the narrow iſthmus, and reſtores a temporary ſtraightneſs to the channel.

Several of the windings of the Ganges and its branches are faſt approaching to this ſtate; and in others, it actually exiſts at preſent. The experience of theſe changes ſhould operate againſt attempting canals of any length, in the higher parts of the country; and I much doubt, if any in the lower parts would long continue navigable. During eleven years of my reſidence in Bengal, the outlet or head of the Jellinghy River was gradually removed three quarters of a mile farther down: and by two ſurveys of a part of the adjacent bank of the Ganges, taken about the diſtance of nine years from each other, it appeared that the breadth of an Engliſh mile and a half had been taken away. This is, however, the moſt rapid change that I have noticed; a mile in ten or twelve years being the uſual rate of incroachment, in places where the current ſtrikes with the greateſt force, namely, where two adjoining reaches approach neareſt to a right angle. In ſuch ſituations it not unfrequently excavates gulfs * of conſiderable length within the bank. Theſe gulfs are in the direction of the ſtrongeſt parts of the ſtream; and are, in fact, the *young ſhoots* (if I may ſo expreſs myſelf) which in time ſtrike out and become branches of the river; for we generally find them at thoſe turnings that have the ſmalleſt angles †.

Two cauſes, widely different from each other, occaſion the meandering courſes of rivers; the one, the irregularity of the ground through which they run, which obliges them to wander in queſt of a declivity; the other, the looſeneſs of the ſoil, which yields to the friction of the border of the ſtream. The meanders in the firſt caſe, are, of courſe, as digreſſive and irregular as the ſurface they are projected on: but, in the latter, they are ſo far reducible to rule, that rivers of unequal bulk will, under ſimilar circumſtances, take a circuit to wind in, whoſe extent is in proportion to their reſpective breadths: for I have obſerved, that

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* The Count DE BUFFON adviſes the digging of ſuch gulfs in the banks of ordinary rivers, with a view to divert the current, when bridges or other buildings are endangered by it.

† The courſes of theſe branches at the efflux, generally, if not always, become retrograde to the courſe of the river: for, a ſand bank accumulating at the upper point of ſeparation, gives an oblique direction upwards, to the ſtream, which would otherwiſe run out at right angles. This ſand bank being always on the increaſe, occaſions a corroſion of the oppoſite bank; and by this means all, or moſt of the outlets have a progreſſive motion downwards; as I have before remarked of the Jellinghy River, in the foregoing page.

when a branch of the Ganges is fallen ſo low as to occupy only a part of its bed, it no longer continues in the line of its old courſe; but works itſelf a new channel, which winds from ſide to ſide acroſs the former one. I have obſerved too, that in two ſtreams, of equal ſize, that which has the ſloweſt current has alſo the ſmalleſt windings: for as theſe (in the preſent caſe) are ſolely owing to the depredations made on the banks by the force of the current; ſo the extent of theſe depredations, or, in other words, the dimenſions of the windings, will be determined by the degree of force acting on the banks.

The windings of the Ganges in the plains, are, doubtleſs, owing to the looſeneſs of the ſoil: and (I think) the proof of it is, that they are perpetually changing; which thoſe, originally induced by an inequality of ſurface, can ſeldom, or never do*.

I can eaſily ſuppoſe, that if the Ganges was turned into a ſtraight canal, cut through the ground it now traverſes in the moſt winding parts of its courſe, its ſtraightneſs would be of ſhort duration. Some yielding part of the bank, or that which happened to be the moſt ſtrongly acted on, would firſt be corroded or diſſolved: thus a bay or cavity would be formed in the ſide of the bank. This begets an inflection of the current, which, falling obliquely on the ſide of the bay, corrodes it inceſſantly. When the current has paſſed the innermoſt part of the bay, it receives a new direction, and is thrown obliquely towards the oppoſite ſide of the canal, depoſiting in its way the matter excavated from the bay, and which begins to form a ſhallow or bank contiguous to the border of the canal. Here then is the origin of ſuch windings as owe their exiſtence to the nature of the ſoil. The bay, ſo corroded, in time becomes large enough to give a new direction to the body of the canal: and the matter excavated from the bay is ſo diſpoſed as to aſſiſt in throwing the current againſt the oppoſite bank, where a proceſs, ſimilar to that I have been deſcribing, will be begun.

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* It has been remarked, that the courſes of rivers become more winding as they approach the ſea. This, I believe, will only hold good in ſuch as take the latter part of their courſe through a ſandy ſoil. In the Ganges, and other rivers ſubject to conſiderable variations in the bulk of their ſtreams, the beſt marks of the vicinity of the ſea, are, the lowneſs of the river banks, and the increaſing muddineſs of the ſhallows in its bed.

The action of the current on the bank will alſo have the effect of deepening the border of the channel near it; and this again increaſes the velocity of the current in that part. Thus would the canal gradually take a new form, till it became what the river now is. Even when the windings have leſſened the deſcent one half, we ſtill find the current too powerful for the banks to withſtand it.

There are not wanting inſtances of a total change of courſe in ſome of the Bengal rivers *. The Coſa River (equal to the Rhine) once ran by Purneah, and joined the Ganges oppoſite Rajemal. Its junction is now 45 miles higher up. Gour, the ancient capital of Bengal, ſtood on the banks of the Ganges.

Appearances favour very ſtrongly the opinion, that the Ganges had its former bed in the tract now occupied by the lakes and moraſſes between Nattore and Jaffiergunge, ſtriking out of its preſent courſe at Bauleah, and paſſing by Pootyah. With an equal degree of probability (favoured by tradition) we may trace its ſuppoſed courſe by Dacca, to a junction with the Burrampooter or Megna near Fringybazar; where the accumu-

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* The Mootyjyl lake is one of the windings of a former channel of the Coſſimbuzar River.

lation of two ſuch mighty ſtreams probably ſcooped out the preſent amazing bed of the Megna *.

In tracing the ſea coaſt of the Delta, we find no leſs than eight openings; each of which, without heſitation, one pronounces to have been in its time the principal mouth of the Ganges. Nor is the occaſional deviation of the principal branch, probably, the only cauſe of fluctuation in the dimenſions of the Delta. One obſerves, that the Deltas of capital rivers (the tropical ones particularly) encroach upon the ſea. Now, is not this owing to the mud and ſand brought down by the rivers, and gradually depoſited, from the remoteſt ages down to the preſent time? The rivers, we know, are loaded with mud and ſand at their entrance into the ſea; and we alſo know, that the ſea recovers its tranſparency at the diſtance of twenty leagues from the coaſt; which can only ariſe from the waters having precipitated their earthy particles within that ſpace. The ſand and mud banks at this time, extend twenty miles off ſome of the iſlands in the mouths of the Ganges and Burrampooter; and in many places riſe within a few feet of the ſurface. Some future generation will probably ſee theſe banks riſe above water, and ſucceeding ones poſſeſs and cultivate them! Next to earthquakes, perhaps the floods of the tropical rivers produce the quickeſt alterations in the face of our globe. Extenſive iſlands are formed in the channel of the Ganges, during a period far ſhort of that of a man's life; ſo that the whole proceſs lies

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* Megna and Burrampooter are names belonging to the ſame river in different parts of its courſe. The Megna falls into the Burrampooter; and, though a much ſmaller river, communicates its name to the other during the reſt of its courſe.

within the compaſs of his obſervation *. Some of theſe iſlands, four or five miles in extent, are formed at the angular turnings of the river, and were originally large ſand banks thrown up round the points (in the manner before deſcribed) but afterwards inſulated by breaches of the river. Others are formed in the ſtraight parts of the river, and in the middle of the ſtream; and owe their origin to ſome obſtruction lurking at the bottom. Whether this be the fragments of the river bank; a large tree ſwept down from it; or a ſunken boat; it is ſufficient for a foundation: and a heap of ſand is quickly collected below it. This accumulates amazingly faſt: in the courſe of a few years it peeps above water, and having now uſurped a conſiderable portion of the channel, the river borrows on each ſide to ſupply the deficiency in its bed; and in ſuch parts of the river we always find ſteep banks on both ſides †. Each periodical flood brings an addition of matter to this growing iſland; increaſing it in height as well as extenſion, until its top is perfectly on a level with the banks that include it: and at that period of its growth it has mould enough on it for the purpoſes of cultivation, which is owing to the mud left on it when the waters ſubſide, and is indeed a part of the œconomy which nature obſerves in fertilizing the lands in general.

Whilſt the river is forming new iſlands in one part, it is ſweeping away old ones in other parts. In the progreſs of this deſtructive operation, we have opportunities of obſerving, by means of the ſections of the falling bank, the regular diſtribution

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* Accordingly, the laws reſpecting alluvion are aſcertained with great preciſion.

† This evidently points out the means for preventing encroachments on a river bank in the ſtraight parts of its courſe, *viz.* to remove the ſhallows in the middle of its channel.

bution of the ſeveral ſtrata of ſand and earths, lying above one another in the order in which they decreaſe in gravity. As they can only owe this diſpoſition to the agency of the ſtream that depoſited them, it would appear, that theſe ſubſtances are ſuſpended at different heights in the ſtream, according to their reſpective gravities. We never find a ſtratum of earth under one of ſand; for the muddy particles float neareſt the ſurface *. I have counted ſeven diſtinct ſtrata in a ſection of one of theſe iſlands. Indeed, not only the iſlands, but moſt of the river banks wear the ſame appearance: for as the river is always changing its preſent bed, and verging towards the ſite of ſome former one now obliterated, this muſt neceſſarily be the caſe.

As a ſtrong preſumptive proof of the wandering of the Ganges from the one ſide of the Delta to the other, I muſt obſerve, that there is no appearance of *virgin* earth between the Tiperah Hills on the eaſt, and the province of Burdwan on the weſt; nor on the north till we arrive at Dacca and Bauleah. In all the ſections of the numerous creeks and rivers in the Delta, nothing appears but ſand and black mould in regular ſtrata, till we arrive at the clay that forms the lower part of their beds. There is not any ſubſtance ſo coarſe as gravel either in the Delta or nearer the ſea than 400 miles †, where a rocky point, a part of the baſe of the neighbouring hills, projects into the river: but out of the vicinity of the great rivers the ſoil is either red, yellow, or of a deep brown.

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* A glaſs of water taken out of the Ganges, when at its height, yields about one part in four of mud. No wonder then that the ſubſiding waters ſhould quickly form a ſtratum of earth; or that the Delta ſhould encroach upon the ſea!

† At Oudanulla.

I come now to the particulars of the annual ſwelling and overflowing of the Ganges *.

It appears to owe its increaſe as much to the rain water that falls in the mountains contiguous to its ſource, and to the ſources of the great northern rivers that fall into it, as to that which falls in the plains of Hindooſtan; for it riſes fifteen feet and a half out of thirty-two (the ſum total of its riſing) by the latter end of June: and it is well known, that the rainy ſeaſon does not begin in moſt of the flat countries till about that time. In the mountains it begins early in † April; and by the latter end of that month, when the rain-water has reached Bengal, the rivers begin to riſe, but by very ſlow degrees; for the increaſe is only about an inch *per* day for the firſt fortnight. It then gradually augments to two and three inches before any quantity of rain falls in the flat countries; and when the rain becomes general, the increaſe on a medium is five inches *per* day. By the latter end of July all the lower parts of Bengal, contiguous to the Ganges and Burrampooter, are overflowed, and form an inundation of more than a hundred miles in width; nothing appearing but villages and trees, excepting very rarely the top of an elevated ſpot (the artificial mound of ſome deſerted village) appearing like an iſland.

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* An opinion has long prevailed, that the ſwelling of the Ganges, previous to the commencement of the rainy ſeaſon in the flat countries, is in a great meaſure owing to the melting of the ſnow in the mountains. I will not go ſo far as totally to diſallow the fact; but can by no means ſuppoſe, that the quantity of ſnow water bears any proportion to the increaſe of the river.

† The vaſt collection of vapours, wafted from the ſea by the ſoutherly or ſouth-weſt monſoon, are ſuddenly ſtopped by the lofty ridge of mountains that runs from eaſt to weſt through Thibet. It is obvious, that the accumulation and condenſation of theſe vapours, muſt firſt happen in the neighbourhood of the obſtacle; and ſucceſſively in places more remote, as freſh ſupplies arrive to fill the atmoſphere. Hence the priority of commencement of the rainy ſeaſon in places that lie neareſt the mountains.

All the rivers that are ſituated within the limits of the monſoons, or ſhifting trade winds, are ſubject to overflowings at annually ſtated periods, like the Ganges: and theſe periods return during the ſeaſon of the ſoutherly wind, that being the only wind which brings vapours from the ſea; and this being periodical, the falls of rain muſt neceſſarily be ſo too.

The northerly wind, which blows only over the land, is dry; for no rain (except caſual ſhowers) falls during the continuance of that monſoon.

The inundations in Bengal differ from thoſe in Egypt in this particular, that the Nile owes its floods *entirely* to the rain-water that falls in the mountains near its ſource; but the inundations in Bengal are as much occaſioned by the rain that falls there, as by the waters of the Ganges; and as a proof of it, the lands in general are overflowed to a conſiderable height long before the bed of the river is filled. It muſt be remarked, that the ground adjacent to the river bank, to the extent of ſome miles, is conſiderably higher than the reſt of the country*, and ſerves to ſeparate the waters of the inundation from thoſe of the river until it overflows. This high ground is in ſome ſeaſons covered a foot or more; but the height of the inundation within, varies, of courſe, according to the irregularities of the ground, and is in ſome places twelve feet.

Even when the inundation becomes general, the river ſtill ſhews itſelf, as well by the graſs and reeds on its banks, as by its rapid and muddy ſtream; for the water of the inundation acquires a blackiſh hue, by having been ſo long ſtagnant amongſt graſs and other vegetables: nor does it ever loſe this tinge, which is a proof of the predominancy of the rain water over that of the river; as the ſlow rate of motion of the inundation (which does not exceed half a mile *per* hour) is of the remarkable flatneſs of the country.

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* This property of the bank is well accounted for by Count BUFFON, who imputes it to the precipitation of mud made by the waters of the river, when it overflows. The inundation, ſays he, purifies itſelf as it flows over the plain; ſo that the precipitation muſt be greateſt on the parts neareſt to the margin of the river.

There are particular tracts of land, which, from the nature of their culture, and ſpecies of productions, require leſs moiſture than others; and yet, by the lowneſs of their ſituation, would remain too long inundated, were they not guarded by dikes or dams, from ſo copious an inundation as would otherwiſe happen from the great elevation of the ſurface of the river above them. Theſe dikes are kept up at an enormous expence; and yet do not always ſucceed, for want of tenacity in the ſoil of which they are compoſed.

During the ſwoln ſtate of the river, the tide totally loſes its effect of counteracting the ſtream; and in a great meaſure that of ebbing and flowing, except very near the ſea. It is not uncommon for a ſtrong wind, that blows up the river for any continuance, to ſwell the waters two feet above the ordinary level at that ſeaſon: and ſuch accidents have occaſioned the loſs of whole crops of rice*. A very tragical event happened at Luckipour † in 1763, by a ſtrong gale of wind conſpiring with a high ſpring tide, at a ſeaſon when the periodical flood was within a foot and half of its higheſt pitch. It is ſaid that the waters roſe ſix feet above the ordinary level. Certain it is, that the inhabitants of a conſiderable diſtrict, with their houſes and cattle, were totally ſwept away; and, to aggravate their diſtreſs, it happened in a part of the country which ſcarce produces a ſingle tree for a drowning man to eſcape to.

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* The rice I ſpeak of is of a particular kind; for the growth of its ſtalk keeps pace with the increaſe of the flood at ordinary times, but is deſtroyed by a too ſudden riſe of the water. The harveſt is often reaped in boats. There is alſo a kind of graſs which overtops the flood in the ſame manner, and at a ſmall diſtance has the appearance of a field of the richeſt verdure.

† This place is ſituated about fifty miles from the ſea.

Embarkations of every kind traverſe the inundation: thoſe bound upwards, availing themſelves of a direct courſe and ſtill water, at a ſeaſon when every ſtream ruſhes like a torrent. The wind too, which at this ſeaſon blows regularly from the ſouth-eaſt *, favours their progreſs; inſomuch, that a voyage, which takes up nine or ten days by the courſe of the river when confined within its banks, is now effected in ſix. Huſbandry and grazing are both ſuſpended; and the peaſant traverſes in his boat, thoſe fields which in another ſeaſon he was wont to plow; happy that the elevated ſite of the river banks place the herbage they contain, within his reach, otherwiſe his cattle muſt periſh.

The following is a table of the gradual increaſe of the Ganges and its branches, according to obſervations made at Jellinghy and Dacca.

At Jellinghy.			At Dacca.	
	Ft.	In.	Ft.	In.
In May it roſe -	6	0	2	4
June - - -	9	6	4	6
July - - -	12	6	5	6
In the firſt half of Auguſt	4	0	1	11
	32	0	14	3

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* Although in the gulf or bay of Bengal the monſoon blows from the S.S.W. and S.W. yet in the eaſtern and northern parts of Bengal it blows from the S.E. or E.S.E.

Theſe obſervations were made in a ſeaſon, when the waters roſe rather higher than uſual; ſo that we may take 31 feet for the medium of the increaſe.

The inundation is nearly at a ſtand for ſome days preceding the middle of Auguſt, when it begins to run off; for although great quantities of rain fall in the flat countries, during Auguſt and September, yet, by a partial ceſſation of the rains in the mountains, there happens a deficiency in the ſupplies neceſſary to keep up the inundation *. The quantity of the daily decreaſe of the river is nearly in the following proportion: during the latter half of Auguſt, and all September, from three to four inches; from September to the end of November, it gradually leſſens from three inches to an inch and a half; and from November to the latter end of April, it is only half an inch *per* day at a medium. Theſe proportions muſt be underſtood to relate to ſuch parts of the river as are removed from the influence of the tides; of which more will be ſaid by and by. The decreaſe of the inundation does not always keep pace with that of the river, by reaſon of the height of the banks; but after the beginning of October, when the rain has nearly ceaſed, the remainder of the inundation goes off quickly by evaporation, leaving the lands highly manured, and in a ſtate fit to receive the ſeed, after the ſimple operation of plowing.

There is a circumſtance attending the increaſe of the Ganges, and which, I believe, is little known or attended to; becauſe few people have made experiments on the heights to which the

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* I have ſtated the middle of Auguſt for the period when the waters begin to run off; and in general it happens with more regularity than the viciſſitudes of the ſeaſons do. But there are exceptions to it; for in the year 1774 the rivers kept up for near a month after the uſual time.

periodical flood riſes in different places. The circumſtance I allude to, is, the difference of the quantity of the increaſe (as expreſſed in the foregoing table) in places more or leſs remote from the ſea. It is a fact, confirmed by repeated experiments, that from about the place where the tide commences, to the ſea, the height of the periodical increaſe diminiſhes gradually, until it totally diſappears at the point of confluence. Indeed, this is perfectly conformable to the known laws of fluids: the Ocean preſerves the ſame level at all ſeaſons (under ſimilar circumſtances of tide) and neceſſarily influences the level of all the waters that communicate with it, unleſs precipitated in the form of a cataract. Could we ſuppoſe, for a moment, that the increaſed column of water, of 31 feet perpendicular, was continued all the way to the ſea, by ſome preternatural agency: whenever that agency was removed, the head of the column would diffuſe itſelf over the Ocean, and the remaining parts would follow, from as far back as the influence of the Ocean extended; forming a ſlope, whoſe perpendicular height would be 31 feet. This is the preciſe ſtate in which we find it. At the point of junction with the ſea, the height is the ſame in both ſeaſons at equal times of the tide. At Luckipour there is a difference of about ſix feet between the heights in the different ſeaſons; at Dacca, and places adjacent, 14; and near Cuſtee, 31 feet. Here then is a regular ſlope; for the diſtances between the places bear a proportion to the reſpective heights. This ſlope muſt add to the rapidity of the ſtream; for, ſuppoſing the deſcent to have been originally four inches *per* mile, this will increaſe it to about five and an half. Cuſtee is about 240 miles from the ſea, by the courſe of the river; and the ſurface of the river there, during the dry ſeaſon, is about 80 feet above the level of the ſea at high water *. Thus far does the Ocean manifeſt its dominion in both ſeaſons: in the one by the ebbing and flowing of its tides; and in the other by depreſſing the periodical flood, till the ſurface of it coincides as nearly with its own, as the deſcent of the channel of the river will admit †.

Similar circumſtances take place in the Jellinghy, Hoogly, and Burrampooter Rivers; and, I ſuppoſe, in all others that are ſubject either to periodical or occaſional ſwellings.

Not only does the flood diminiſh near the ſea, but the river banks diminiſh in the ſame proportion; ſo that in the dry ſeaſon the height of the periodical flood may be known by that of the bank.

I am aware of an objection that may be made to the above ſolution; which is, that the lowneſs of the banks in places near the ſea, is the true reaſon why the floods do not attain ſo conſiderable a height, as in places farther removed from it, and where the banks are high; for that the river, wanting a bank to confine it, diffuſes itſelf over the ſurface of the country. In

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[*] The tides in the River Amazons are perceptible at 600 miles above its mouth; but at an elévation of only 90 feet, according to M. DE CONDAMINE. It remains to be told what the ſtate of the river was at the time of making the experiment; becauſe the land-floods have the effect of ſhortening the limits of the tide's way.

[†] The Count DE BUFFON has ſlightly mentioned this circumſtance attending the ſwelling of rivers; but imputes it to the increaſed velocity of the current, as the river approaches the ſea: which, ſays he, carries off the inundation ſo quick, as to abate its height. Now (with the utmoſt deference to ſo great an authority) I could never perceive, that the current, either in the Ganges, or any other river, was ſtronger near the ſea than at a diſtance from it. Even if we admit an acceleration of the current during the ebb tide, the flux retards it in ſo conſiderable a degree, as at leaſt to counter balance the effects produced by the temporary increaſe of velocity.

anſwer to this, I ſhall obſerve, that it is proved by experiment, that at any given time, the quantity of the increaſe in different places, bears a juſt proportion to the ſum total of the increaſe in each place reſpectively: or, in other words, that when the river has riſen three feet at Dacca, where the whole riſing is about 14 feet; it will have roſe upwards of ſix feet and a half at Cuſtee, where it riſes 31 feet in all.

The quantity of water diſcharged by the Ganges, in one ſecond of time, during the dry ſeaſon, is 80,000 cubic feet; but in the place where the experiment was made, the river, when full, has thrice the volume of water in it; and its motion is alſo accelerated in the proportion of 5 to 3: ſo that the quantity diſcharged in a ſecond at that ſeaſon is 405,000 cubic feet. If we take the medium the whole year through, it will be nearly 180,000 cubic feet in a ſecond.

THE Burrampooter, which has its ſource from the oppoſite ſide of the ſame mountains that give riſe to the Ganges, firſt takes its courſe eaſtward (or directly oppoſite to that of the Ganges) through the country of Thibet, where it is named Sanpoo or Zanciu, which bears the ſame interpretation as the Gonga of Hindooſtan: namely, the River. The courſe of it through Thibet, as given by Father DU HALDE, and formed into a map by Mr. D'ANVILLE, though ſufficiently exact for the purpoſes of general geography, is not particular enough to aſcertain the preciſe length of its courſe. After winding with a rapid current through Thibet, it waſhes the border of the territory of Laſſa (in which is the reſidence of the grand Lama) and then deviating from an eaſt to a ſouth-eaſt courſe, it approaches within 220 miles of Yunan, the weſternmoſt province of China. Here it appears, as if undetermined whether to attempt a paſſage to the ſea by the Gulf of Siam, or by that of Bengal; but ſeemingly determining on the latter, it turns ſuddenly to the weſt through Aſſam, and enters Bengal on the north-eaſt. I have not been able to learn the exact place where it changes its name; but as the people of Aſſam call it Burrampoot, it would appear, that it takes this name on its entering Aſſam. After its entry into Bengal, it makes a circuit round the weſtern point of the Garrow Mountains; and then, altering its courſe to ſouth, it meets the Ganges about 40 miles from the ſea.

Father DU HALDE expreſſes his doubts concerning the courſe that the Sanpoo takes after leaving Thibet, and only ſuppoſes generally that it falls into the gulf of Bengal. M. D'ANVILLE, his geographer, with great reaſon ſuppoſed the Sanpoo and Ava River to be the ſame: and in this he was juſtified by the information which his materials afforded him: for the Burrampooter was repreſented to him, as one of the inferior ſtreams that contributed its waters the Ganges, and not as its equal or ſuperior; and this was ſufficient to direct his reſearches, after the mouth of the Sanpoo River, to ſome other quarter. The Ava River, as well from its bulk, as the bent of its courſe for ſome hundred miles above its mouth, appeared to him to be a continuation of the river in queſtion: and it was accordingly deſcribed as ſuch in his maps, the authority of which was juſtly eſteemed as deciſive; and, till the year 1765, the Burrampooter, as a capital river, was unknown in Europe.

On tracing this river in 1765, I was no leſs ſurprized, at finding it rather larger than the Ganges, than at its courſe previous to its entering Bengal. This I found to be from the eaſt; although all the former accounts repreſented it as from the north: and this unexpected diſcovery ſoon led to enquiries, which furniſhed me with an account of its general courſe to within 100 miles of the place where DU HALDE left the Sanpoo. I could no longer doubt, that the Burrampooter and Sanpoo were one and the ſame river: and to this was added the poſitive aſſurances of the Aſſamers, "That *their* river came "from the north-weſt, through the Bootan mountains." And to place it beyond a doubt, that the Sanpoo River is not the ſame with the river of Ava, but that this laſt is the great Nou Kian of Yunan; I have in my poſſeſſion a manuſcript draught of the Ava River, to within 150 miles of the place where DU HALDE leaves the Nou Kian, in its courſe towards Ava; together with very authentic information that this river (named Irabattey by the people of Ava) is navigable from the city of Ava into the province of Yunan in China*.

The Burrampooter, during a courſe of 400 miles through Bengal, bears ſo intimate a reſemblance to the Ganges, except in one particular, that one deſcription may ſerve for both. The exception I mean is, that, during the laſt 60 miles before its junction with the Ganges, it forms a ſtream which is regularly from four to five miles wide, and but for its freſhneſs might paſs for an arm of the ſea. Common deſcription fails in an attempt to convey an adequate idea of the grandeur of this magnificent object; for,

—— Scarce the muſe
Dares ſtretch her wing o'er this enormous maſs
Of ruſhing water; to whoſe dread expanſe,
Continuous depth, and wond'rous length of courſe,

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* The courſes of the Burrampooter and Ganges, as well as that of the Ava River, from Yunan to the ſea, will ſhortly be deſcribed in a large ſheet map of Hindooſtan.

Our floods are rills ———
Thus pouring on, it proudly ſeeks the deep,
Whoſe vanquiſh'd tide, recoiling from the ſhock,
Yields to this liquid weight ———

THOMSON's Seaſons.

I have already endeavoured to account for the ſingular breadth of the Megna, by ſuppoſing that the Ganges once joined it where the Iſſamutty now does; and that their joint waters ſcooped out its preſent bed. The preſent junction of theſe two mighty rivers below Luckipour, produces a body of running freſh water, hardly to be equalled in the old hemiſphere, and, perhaps, not exceeded in the new. It now forms a gulf interſperſed with iſlands, ſome of which rival, in ſize and fertility, our Iſle of Wight. The water at ordinary times is hardly brackiſh at the extremities of theſe iſlands; and, in the rainy ſeaſon, the ſea (or at leaſt the ſurface of it) is perfectly freſh to the diſtance of many leagues out.

The *Bore* (which is known to be a ſudden and abrupt influx of the tide into a river or narrow ſtrait) prevails in the principal branches of the Ganges, and in the Megna; but the Hoogly River, and the paſſages between the iſlands and ſands ſituated in the gulf, formed by the confluence of the Ganges and Megna, are more ſubject to it than the other rivers. This may be owing partly, to their having greater embouchures in proportion to their channels, than the others have, by which means a larger proportion of tide is forced through a paſſage comparatively ſmaller; and partly, to there being no capital openings near them, to draw off any conſiderable portion of the accumulating tide. In the Hoogly or Calcutta River, the *Bore* commences at Hoogly Point (the place where the river firſt contracts itſelf)

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and is perceptible above Hoogly Town; and ſo quick is its motion, that it hardly employs four hours in travelling from one to the other, although the diſtance is near 70 miles. At Calcutta, it ſometimes occaſions an inſtantaneous riſe of five feet: and both here, and in every other part of its track, the boats, on its approach, immediately quit the ſhore, and make for ſafety to the middle of the river.

In the channels, between the iſlands in the mouth of the Megna, &c. the height of the *Bore* is ſaid to exceed twelve feet; and is ſo terrific in its appearance, and dangerous in its conſequences, that no boat will venture to paſs at ſpring tide. After the tide is fairly paſt the iſlands, no veſtige of a *Bore* is ſeen, which may be owing to the great width of the Megna, in compariſon with the paſſages between the iſlands; but the effects of it are viſible, enough by the ſudden riſing of the tides.

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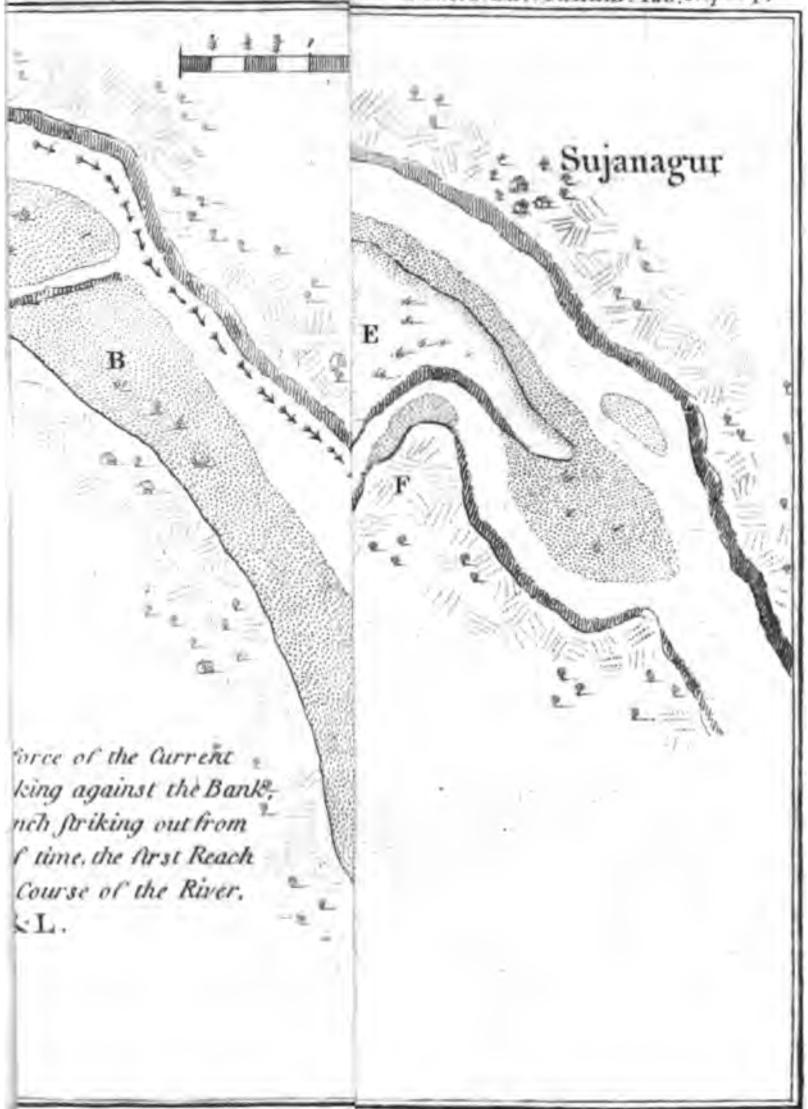

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further explanation
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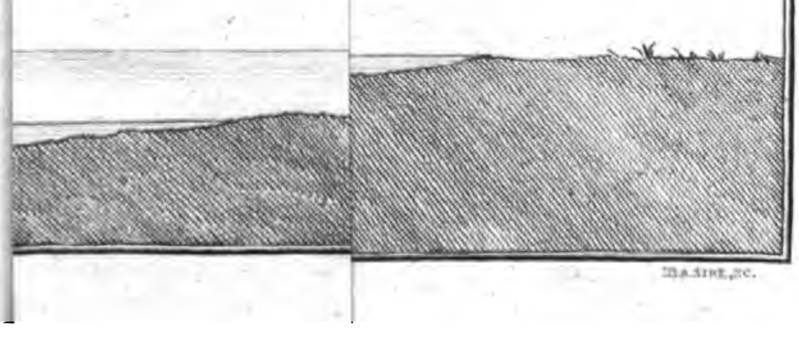

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X. *Aſtronomical Obſervations on the Rotation of the Planets round their Axes, made with a View to determine whether the Earth's diurnal Motion is perfectly equable. In a Letter from Mr.* William Herſchel *of* Bath *to* William Watſon, *M. D. F. R. S.*

Read January 11, 1781.

SIR, Bath, October 18, 1780.

THE various motions of the planet we inhabit; the annual revolution in its orbit; the diurnal rotation round its axis; the menſtrual motion round the common center of gravity of the moon and earth; the preceſſion of the equinoctial points; the diminution of the obliquity of the ecliptic; the nutation of the earth's axis: in ſhort, every one of the motions that ariſe from the actions of the ſun, moon, and planets, combined with the ſpheroidical figure of the earth, and the projectile and rotatory motions firſt impreſſed upon it, have all been conſidered by aſtronomers, and their real and apparent inequalities inveſtigated. And to the great honour of modern aſtronomers it muſt be confeſſed, that no ſcience has ever made ſuch conſiderable ſtrides towards perfection in ſo ſhort a time as aſtronomy has done ſince the invention of the teleſcope.

There is one of the motions of the earth however which, it ſeems, has hitherto eſcaped the ſcrutiny of obſervers; I mean the diurnal rotation round its axis. The principal reaſon

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why this has not been looked into, is probably the difficulty of finding a proper ſtandard to meaſure it by; ſince it is itſelf uſed as the ſtandard by which we meaſure all the other motions. We have, indeed, no cauſe to ſuſpect any very material periodical irregularity, either diurnal, menſtrual, or annual; for the great perfection of our preſent time-pieces would have diſcovered any conſiderable deviation from that equability which we have hitherto aſcribed to the diurnal motion of the earth. And yet, it is not perhaps altogether impoſſible but that inequalities may exiſt in this motion which, in an age where obſervations are carried to ſuch a degree of refinement, may be of ſome conſequence.

To ſhew how far time-keepers, though ever ſo perfect, are from being a proper, or at leaſt a ſufficient, ſtandard to examine the diurnal motion of the earth by, I may aſk, whether it is probable, that any clock would have diſcovered to us the aberration of the fixed ſtars? And yet that aberration produces a change in longitude, and of conſequence in right aſcenſion, which cauſes an annual irregularity in a ſtar's coming to the meridian, which a time-piece, were it a ſufficient ſtandard, would ſoon have diſcovered, and which we might have attributed to an inequality of the earth's diurnal motion, had we not been acquainted with its real cauſe. And if we were to find out any apparent irregularity, acceleration, or retardation, ſhould we not much rather ſuſpect the clock than the diurnal motion? I may therefore venture to ſay, that the aberration of the fixed ſtars, though attended with the above mentioned conſequence, would for ever have remained a ſecret to us, if it had not been found out by other methods than time-keepers.

Now, if time-pieces do fail us in this critical caſe, where we ſtand in the greateſt need of their aſſiſtance, it is almoſt in-vain to expect any help from another quarter; for what mechanical movement on earth, or motion of the heavens, is there that can meaſure out ſuch equal portions of time as we require to compare the diurnal motion of the earth to? However, to proceed, ſince we have already great proofs that the diurnal motion of the earth is, if not perfectly equable, at leaſt more ſo than any other motion we are acquainted with, it will not appear abſurd to ſuppoſe the diurnal rotation of the other planets to be ſo likewiſe. This ſuggeſted to me the thoughts of eſtimating the diurnal motion of one planet very exactly by that of another, making each the ſtandard of the other. In this manner we may obtain a comparative view, by which future aſtronomers, if they ſhall hereafter be inclined to purſue the ſubject, may be enabled to make ſome eſtimate of the general equability of the rotatory motions of the planets. For if in length of time they ſhould perceive ſome ſmall retardation in the diurnal motion of a planet occaſioned by ſome reſiſtance of a very ſubtle medium in which the heavenly bodies perhaps move; or, on the other hand, if there ſhould be found an acceleration from ſome cauſe or other, they might then aſcribe the alteration either to the diurnal motion of the earth, or to the gyration of the other planet, according as circumſtances, or obſerved phænomena, ſhould make one or the other of theſe opinions moſt probable.

Now, this method of comparing together different rotations of ſeveral planets, ſimple as it may appear, was not without ſome difficulties. In the firſt place it was evident, that the common account of their diurnal motions *, which makes that

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* Venus ſpatio 23 horarum gyrationem circa axem ab occidente in orientem perficit. Mars ſimilem rotationem, horis 24 min. 40 abſolvit. Macularum revolutionibus ſæpius obſervatis, Ds. CASSINI comperuit periodum Jovis circa proprium Axem eſſe horarum 9, minutorum 56. KEILL, Aſt. Lect. V.

of Jupiter 9 h. 56′, of Mars 24 h. 40′, how true ſoever it may be in a general way, was much too inaccurate for this critical purpoſe. The gyration of Venus was ſtill leſs to be depended upon; being only noted to the hour without the minutes: it became, therefore, neceſſary to proceed to obſervations of a more determinate kind. From what I had already ſeen of the rotation of the planets, I concluded, that Mars on ſeveral accounts would be the moſt eligible planet for my purpoſe: for the ſpots on Jupiter change ſo often that it is not eaſy, if at all poſſible, to aſcertain the identity of the ſame appearance, for any conſiderable length of time. Nor do the dark ſpots only change their place, which may be ſuppoſed to be large black congeries of vapours and clouds ſwimming in the atmoſphere of Jupiter; but alſo the bright ſpots, though they may adhere firmly to the body of Jupiter, may undergo ſome apparent change of ſituation by being differently covered or uncovered on one ſide or the other, by alterations in the belts. It will be ſeen hereafter, that I have obſerved the revolution of a very bright ſpot, not ſuſpected of any change of ſituation, to be firſt, by one ſet of obſervations, at the rate of 9 h. 51′ 45″,6; and afterwards, by another ſet immediately following at the rate of 9 h. 50′ 48″.

As the principal belts on Jupiter are equatorial, and as we have certain conſtant winds upon our planet, eſpecially near the equator, that regularly, for certain periods, blow the ſame way *, it is eaſily ſuppoſed, that they may form equatorial belts by gathering together the vapours which ſwim in our atmoſphere, and carrying them about in the ſame direction. This will, by analogy, account for all the irregularities of Ju-

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* See Acta Eruditorum, 1687. Dr. HALLEY's Account of periodical Winds.

piter's revolutions, deduced from ſpots on his diſk that may have changed their ſituation; for if we ſuppoſe the rotation of Jupiter, according to CASSINI, to be 9 h. 56′, then ſome ſpots that I have obſerved muſt have been carried through about 60° of Jupiter's equator in 22 of his revolutions or days. This would certainly be a very great velocity in the clouds, which is, however, not unparalleled by what has happened in our own atmoſphere.

But to return to my purpoſe: on the planet Mars we ſee ſpots of a different nature; their conſtant and determined ſhape, as well as remarkable colour, ſhew them to be permanent and faſtened to the body of the planet. Theſe will give the revolution of his equator to a great certainty, and by a great number of revolutions, to a very great exactneſs alſo. Suppoſing then, that, by a method I ſhall hereafter deſcribe, we can determine whether a ſpot on the diſk of Mars is, or is not, in the line which joins the center of the earth and the center of that planet, to half an hour's time with certainty (I believe ten or twelve minutes will be found ſufficient for that purpoſe), in this caſe we ſhall in 30 days have the revolution true to a minute; and, by continuing theſe obſervations for three months, we ſhall have it to 20″. When we are ſo far certain, we can eaſily arrive to a much greater degree of exactneſs; for as we now can no longer miſtake a whole revolution, if we take the time of any particular ſpot's being in the line which joins the centers of the planets during one oppoſition of Mars, and take the ſame again at or near the next oppoſition, we ſhall have an interval of about 780 days, which will give the diurnal motion of that planet true to about 2″. The next oppoſition will give it to one, and ſo forth; by which means, and by taking a proper number of ſuch periods, we may determine the rotation of

Mars to as great an exactneſs as we ſhall think neceſſary for the purpoſe of our comparative view.

Had ſuch obſervations as theſe been made two thouſand, or perhaps only ſo many hundred years ago, we might now, by repeating them, moſt probably become acquainted with ſome curious minute changes of the ſolar ſyſtem that have hitherto paſſed unnoticed.

There is a certain circumſtance which would almoſt create a ſuſpicion that there has been ſome retardation in the diurnal motion of the earth. The difference between the equatorial and polar diameters of the earth, by actual meaſurement, has been found to be about 36 Engliſh miles and 9 tenths; but, by a calculation wherein the preſent rotation is made uſe of, it will only amount to about 33 miles and 8 tenths: from which it ſhould ſeem probable, that when the earth aſſumed the preſent form, the diurnal rotation was ſomewhat quicker than it is at preſent, by which means the centrifugal force bore a greater proportion to the force of gravity to which it is contrary, and thus occaſioned a higher elevation of the equatorial parts. But I would not lay much ſtreſs upon this argument; for, in the calculation, it has been ſuppoſed, that the earth is nearly of an equal denſity at the ſurface and towards the center, which it ſeems is not agreeable to ſome late curious experiments and calculations that have been made under the conduct of the Aſtronomer Royal upon the attraction of a mountain *, the reſult of which ought now to be taken into conſideration, and the calculation repeated. If all the *data* could be exactly depended upon, it would be practicable enough from the laws of

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* See Mr. HUTTON's Account of the Calculations made from the Survey and Meaſures taken at Schehallien, in order to aſcertain the mean Denſity of the Earth. Phil. Tranſ. 1778.

gravity, and the preſent rotation and given form of the earth, to find the centrifugal force required to produce that form, and thence to ſhew what muſt have been its diurnal motion when it aſſumed the ſame. However, theſe are reſearches that in my preſent ſituation I neither have opportunity nor perhaps ability enough to inveſtigate properly; and which, therefore, I hope ſome of our excellent mathematicians will think worth while to look into.

I ſhall now relate my obſervations on Jupiter and Mars. The teleſcopes I uſed are of my own conſtruction; and are, a twenty-feet Newtonian reflector, a ten-feet reflector of the ſame form, and a ſeven-feet reflector already mentioned in my paper on the mountains of the moon. My time I gained by equal altitudes taken with a braſs quadrant of two-feet radius, carrying a teleſcope which magnifies about 40 times; for the correction of altitudes taken of the ſun I uſed DE LA LANDE's tables. I kept my time by two very good pieces; one having a deal pendulum-rod, the other a compounded one of braſs and iron, both having a proper contrivance not to ſtop when winding up. The rate of going of my clocks I determined by the tranſit of ſtars.

Obſervations on Jupiter in the year 1778.

February 24. Clock 1′ 10″ too ſoon. About 9 o'clock I ſaw a bright belt on one part of the diſk of Jupiter, ſee tab. V. fig. 1.

About 10 o'clock it was advanced as far as the center, fig. 2.

11 h. The white belt ſtill more advanced, fig. 3.

11 h. 25′. It approached towards the edge of the diſk; and at 12 h. was extended all over, as in fig. 4.

February 25. 8 h. The ſame bright belt I obſerved yeſterday extends all over.

8 h. 45′. It is divided by a darkiſh ſpot, ſituated at ſome diſtance from the center, as in fig. 5.

9 h. 5′. The ſmall dark diviſion is advanced a little farther than the center, as in fig. 6.

9 h. 23′. The ſpot is viſibly advanced a conſiderable deal farther.

March 2. 8 h. 2′. The darkiſh ſpot, with ſome alteration in its ſhape, is now in the middle of the diſk, ſee fig. 7.

March 3. 10 h. 34′. The bright belt on the ſouth of the equator is now in the middle; that is to ſay, if a line be drawn perpendicular to the equatorial belt, and through the center, the end of the equatorial belt now touches it, fig. 8.

13 h. 49′. The darkiſh ſpot, in which there has been ſome alteration ſince yeſterday, ſeems now to be in the center, fig. 9.

March 14. The clock altered to true equated time; but the rate of going not changed, being well regulated.

7 h. 35′. The ſpot is now in the center, but does not ſeem quite to fill the white belt; nor is it ſo large and diſtinct as it was before, fig. 10.

April 7. 9 h. 31′. There are three dark ſpots in the equatorial belt nearly in the center, ſee fig. 11.

April 12. 7 h. 50′. The three dark ſpots are in the center. The ſourthernmoſt of the three is nearly quite vaniſhed; the other two are alſo much fainter. They are, however, diſtinct enough to be known, fig. 12.

Obſervations on Jupiter in 1779.

April 14. Clock 52″ too late. 8 h. 48′ A remarkable bright ſpot in the equatorial belt towards the north is in the center, ſee fig. 18.

8 h. 58′. The ſpot is a little paſt the center.

April 19. Clock true mean time. 7 h. 10′. There is a bright ſpot juſt now in the center, which, from its ſhape, I take to be the ſame that was there April 14th.

7 h. 20′. The ſpot is viſibly paſt the center.

April 23. Clock ſhews true time. 9 h. 38′. The ſame bright ſpot is in the center.

9 h. 43′. It is paſt the center. *Memorandum*, my time-piece may be depended upon to a few ſeconds.

It will not be amiſs to obſerve, that the ſpots, as well as a great many other phænomena, were watched as they came on, paſſed over the center, and went off the diſk of Jupiter; but I have only ſelected thoſe obſervations that were neceſſary to my preſent purpoſe.

Comparing together the obſervations that were made in the year 1778, February 24th and March 3d, we obtain an interval of 7 days 34 minutes, which being divided by 17 revolutions made by Jupiter on his axis, we have the time of one ſynodical revolution equal to 9 h. 54′ 56″,4.

The dark ſpot on February 25 was obſerved ſome time before, and alſo juſt after it was paſt the center; therefore I have ſuppoſed it to be in the center about 8 h. 58′: and we have,

	D.	H.	M.
February	25	8	58
March	2	8	2
Divided by 12 rev.	4	23	4

1 revolution = 9 h. 55′ 20″.

February	25	8	58
March	3	13	49
15 revol.	6	4	51

1 revolution = 9 h. 55′ 24″.

February	25	8	58	0	
March	14	7	36	10	allowing 1′ 10″ for the alt. of the clock.
41 revol.	16	22	38	10	

1 revolution = 9 h. 55′ 4″,6.

March	2	8	2
	3	13	49
1 revol.	1	5	47

1 revolution = 9 h. 55′ 40″.

March	2	8	2	0
	14	7	36	10
29 revol.	11	23	34	10

1 revolution = 9 h. 54′ 58″,2.

March

	D.	H.	M.	S.
March	3	13	49	0
	14	7	36	10
Divided by 26 rev.	10	17	47	10

1 revolution = 9 h. 54′ 53″,4.

April	7	9	31
	12	7	50
12 revol.	4	22	29

1 revolution = 9 h. 51′ 35″.

Again, comparing together the obſervations of 1779, which were made with the utmoſt attention to time, we have,

April	14	8	48	52
April	19	7	10	0
12 revol.	4	22	21	8

1 revolution = 9 h. 51′ 45″6.

April	19	7	10
April	23	9	38
10 revol.	4	2	28

1 revolution = 9 h. 50′ 48″.

And taking both together,

April	14	8	48	52
April	23	9	38	0
22 revol.	9	0	49	8

1 revolution = 9 h, 51′ 19″,4.

Theſe ſeveral reſults are ſo exceedingly various, that it is evident Jupiter is not a proper planet for the critical purpoſe of a comparative view of the diurnal motions; nor can this great variety proceed from any inaccuracy in the obſervations: for, in my opinion, it is not well poſſible to make a miſtake in the ſituation of a ſpot that ſhall amount to ſo much as five minutes of time. The obſervation of April 23, 1779, was made with a view to aſcertain this point, when it was found that five minutes of time made a ſenſible difference in the ſituation of a ſpot when near the center.

If we reduce the ſynodical revolutions to ſydereal ones, the reſult will be ſo little different from the above, that I have not thought it worth while to do it in this place. By a compariſon of the different periods it appears, that a ſpot which is carried about in the atmoſphere of Jupiter generally ſuffers an acceleration, or, which is the ſame thing, performs its revolutions by degrees in leſs time than it did at firſt; for the ſpot obſerved in 1778 moved at the following rates. From February 25. to March 2. in 9 h. 55′ 20″; to March 3. nearly the ſame; to March 14. in 9 h. 55′ 4″; from March 2. to March 3. in 9 h. 55′ 40″; to March 14. in 9 h. 54′ 58″; from March 3. to March 14. in 9 h. 54′ 53″. In 1779 a ſpot moved from April 14. to April 19. at the rate of 9 h. 51′ 45″; to April 23. in 9 h. 51′ 24″; and from April 19. to April 24. in 9 h. 50′ 48″; all which is agreeable enough to the theory of equatorial winds, ſince it may probably take up ſometime before a ſpot can acquire a ſufficient velocity to go as faſt as thoſe winds may blow. And, by the by, if Jupiter's ſpots ſhould be obſerved in different parts of his year, and be found in ſome to be accelerated, in others to be retarded, it would almoſt amount to a demonſtration of his monſoons and their periodical changes; but if his

axis

axis ſhould not be inclined enough to his orbit, to occaſion ſuch a change, they may probably always blow in the ſame direction.

Obſervations on Mars in the year 1777.

Twenty-feet Newtonian reflector; power 300.

April 8. 7 h. 30′. I obſerved two ſpots upon Mars, with a bright belt or partition between them. The belt was not very well defined, ſee tab. VI. fig. 14.

9 h. 30′. The ſpots are advanced, and more ſpotted parts are viſible, fig. 15.

10 h. The revolution of Mars on his axis is now very evident, fig. 16.

April 17. Ten-feet Newtonian reflector; power about 211. 7 h. 50′. Mars appeared as in fig. 17. At *a* and *b* there were two bright ſpots, ſo luminous that they ſeemed to project beyond the diſk. At *c* and *d* there were two very dark ſpots, joined by a leſſer black line in the middle, which however was croſſed at *e* and *f* by a very faint whitiſh partition.

April 26. Ten-feet reflector; power 211.

9 h. 5′. The ſpots on the planet are very faint, and much about as in fig. 18.

April 27. Ten-feet reflector; power 324.

8 h. 40′. The evening very fine: my teleſcope in compleat order. The ſpots as in fig. 19.

Obſervations

Obſervations on Mars in the year 1779.

May 9th. Clock 15″ too faſt; by equal altitudes on the 14th of April, and by the tranſit of a ſtar, is found to loſe 1″,45 *per* day.

11 h. 1′ by the clock, I found the ſituation of the ſpots on Mars as in figure 20; there is a very remarkable dark ſpot not far from the center.

11 h. 30′. The figures are gone from the center.

May 11. Clock 12″ too faſt.

10 h. 18′. The ſame ſpot that was viſible May 9. is on the diſk, the darkeſt place being intirely ſouth-eaſt of the center, ſee fig. 21.

11 h. 43′. The darkeſt part is almoſt arrived at the center, fig. 22.

12 h. 17′. The dark ſpot is with its edge juſt near the center, as in fig. 23.

May 13. Seven-feet reflector; power 222. Clock 9″ too faſt.

11 h. 26′. Mars ſeems now to be in the ſame ſituation he was the 11th, at 10 h. 8′.

May 22. Clock 4″ too ſlow.

12 h. 5′. The figure of May 11th is not on the diſk; but ſome other fainter ſpots are viſible. The air is full of vapours.

June 6. The clock ſet by ten equal altitudes taken to-day, and by the tranſit of δ Scorpii loſes 1″,9 *per* day. What I have perhaps improperly called a tranſit is the occultation of a ſtar paſſing behind the perpendicular edge of a high building at about 40 yards diſtance, obſerved with a fixed teleſcope directed to the place where it vaniſhes.

10 h. 10′. The ſame figure is upon the diſk of Mars which was there April 8, 1777, at 7 h. 30′.

June 15. Clock 17″ too ſlow. 9 h. 45′. The ſame figure is upon Mars that was there May 9. at 11 h. 1′; but it is more advanced. I ſuppoſe it to be the ſame, and in the ſame ſituation, as April 17, 1777, at 7 h. 50′.

June 17. Clock 20″ ſlow.

9 h. 12′. The dark ſpot on Mars is rather more advanced than it was May 11th, at 10 h. 18′.

10 h. The ſpot is viſibly advanced: I ſuppoſe it will take near an hour to come to the center.

10 h. 15′. A very thick fog obſcures the ſky.

11 h. 15′. The ſame darkneſs.

June 19. Clock 22″ too ſlow by the tranſit of δ Scorpii obſerved this evening.

8 h. 40′. The figure on the diſk of Mars appears now to be as it was April 26, 1777, at 9 h. 5′, ſee fig. 18.

11 h. 30′. The figure of May 11. which I have been hitherto watching, is not come to the poſition it was then at 11 h. 43′, but cannot be far from it. I fear, as Mars approaches the horizon, I ſhall not be able to follow him till the figure comes to the center.

11 h. 47′. The ſtate of the air near the horizon is very unfavourable. With much difficulty I can but juſt ſee that the figure is not quite ſo far advanced as it was May 11th, at 11 h. 43′, but can certainly not be above two or three minutes from it.

11 h. 51′. The undulation of the air prevents all further obſervation.

Let us now examine the reſult of the above mentioned obſervations: comparing together the two following ſhort intervals of the year 1779, we have,

	D.	H.	M.	S.
From May	9	11	0	45
to May	11	12	16	48
Divided by 2 revol.	2	1	16	3

Gives 1 revolution = 24 h. 38′ 1″,5.

A ſecond ſmall interval.

May	11	10	17	48
	13	11	25	51
2 revol.	2	1	8	3

1 revolution = 24 h. 34′ 1″,5.

Here we have two very ſhort intervals that agree to 4′, which is more than we could have expected in ſuch ſhort periods of time.

Comparing together obſervations that were made at a greater diſtance, we find,

Firſt monthly period,

May	11	10	17	48	
June	17	9	9	20	allowing 3′ becauſe the obſ. ſays the ſpot was rather more advanced.
36 revol.	36	22	51	32	

1 revolution = 24 h. 38′ 5″,9.

Second monthly period,

May	11	11	42	48	
June	19	11	50	22	allowing 3′ for the time the ſpot would have taken to come to the place mentioned.
38 revol.	39	0	7	34	

1 revolution = 24 h. 38′ 5″,4.

Third monthly period,

	D.	H.	M.	S.
May	13	11	25	51
June	17	9	9	20
34 revol.	34	21	43	29

1 revolution = 24 h. 38′ 20″,3

This laſt is, perhaps, as likely to be near the truth as any, ſince the ſame ſpot was here obſerved for the third time, and therefore its motion become more familiar.

Here we have three longer periods that agree to fifteen ſeconds, which is quite ſufficient for extending the interval of time to thoſe obſervations that were made in the year 1777. But as theſe are the ſynodical revolutions, it will be neceſſary firſt to reduce them to ſydereal rotations.

In figure 24. let us ſuppoſe the orbit of Mars, MABC, to be in the ſame plane with the orbit of the earth, EDFG; and the axis of Mars to be perpendicular to his orbit. Let M, E, *m*, *e*, be the ſituations of Mars and the earth on the 13th of May and 17th of June; then will the line EM, that connects the centers of Mars and the earth, point out the geocentric place of Mars on the 13th of May; and the line *em*, the geocentric place of the ſame planet on the 17th of June. Draw *er* and *ms* parallel to ER; then will *er* point out the geocentric place of Mars on the 13th of May; and the angle *sme* is equal to the angle *mer*. Now, by an ephemeris * the geocentric place of Mars, May 13. at 11 h. 26′ was 7 ſ. 20 d. 59′ 21″;

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* The Nautical Almanac gives the geocentric place of Mars only to every ſixth day; for which reaſon I uſed WHITE's Ephemeris, where it is given for every day, though perhaps not with ſo much exactneſs as I could wiſh.

and on the 17th of June, at 9 h. 9′, it was 7 ſ 12 d. 27′ 22″, by which we obtain the difference or angle $rem = ems =$ 8 d. 31′ 59″.

Now a ſpot on Mars, ſituated in the direction ME, will have made a ſydereal revolution when it returns to the ſame, or a parallel direction *ms*. From which we gather, that the ſpot on the 17th of June, after coming to the line *me*, where it finiſhes the ſynodical revolution, will have to go through an arch of 8 d. 31′ 59″, in order to arrive into the direction of the line *ms*, where it finiſhes the ſydereal rotation. The time it will take to go through this arch, at the ſydereal rate of 24 h. 39′ 20″ to 360 degrees, or 4″,109 *per* minute of a degree, will be 35′ 3″,8; this being divided by the numbers of revolution 34, gives 1′ 1″,8; which, added to 24 h. 38′ 20″,3, gives us 24 h. 39′ 22″,1 for the ſydereal revolution of Mars, as found by the third of the monthly periods. This quantity will help us to find a proper diviſor for the three following long biennial periods.

It is to be obſerved, that Mars has been retrograde in the above example, for which reaſon the meaſure of the angle *ems* was to be added to the ſynodical revolution when we wanted to find the ſydereal rotation; but if he had been direct, or if his place had been more advanced in the ecliptic than that to which we compared it, as at μ, then the line $\mu\sigma$ parallel to EM would be the direction to which the ſpot ſhould return, in order to accompliſh a ſydereal revolution, and therefore the quantity of the angle $\sigma\mu e = \mu e r$, or difference of the geocentric places ought to be ſubtracted from the ſynodical revolution to obtain the ſydereal one.

			D.	M.	S.
Firſt biennial period,	1777,	April	8	7	30
	1779,	June	6	10	10
			789	2	40

The geocentric places of Mars at thoſe times were,

S.	D.	′	″
6	6	31	26
7	13	48	30
1	7	17	4

turned into time at 4″,109 *per* minute of a degree and ſubtracted, becauſe Mars is more advanced in the ecliptic, is

	789	2	40	0
	—	2	33	11,8
Divided by 768 rev.	789	0	6	48,2

1 revolution = 24 h. 39′ 23″,03.

			D.	H.	M.	S.
Second biennial period,	1777,	April	17	7	50	0
	1779,	June	15	9	45	17
			789	1	55	17

	S.	D.	′	″
Geocentric places	6	3	31	27
	7	12	40	23
	1	9	8	56

Turned into time	789	1	55	17
and ſubtracted	—	2	40	52
768 revol.	788	23	14	25

1 revolution = 24 h. 39′ 18″,94.

Third

		D.	H.	M.	S.
Third biennial period,	1777, April	26	9	5	0
	1779, June	19	8	40	22
		783	23	35	22

	S.	D.	′	″
Geocentric places	6	1	24	36
	7	12	31	48
	1	11	7	12

Turned into time	783	23	35	22
and ſubtracted	—	2	45	15,6
763 revol.	783	20	50	6,4

1 revolution = 24 h. 39′ 23″,04.

As theſe three periods are ſupported by obſervations of equal validity, I ſhall take a mean of them all for the neareſt approximation to the true ſydereal revolution of Mars on his axis, which therefore is 24 h. 39′ 21″,67.

It remains now only to ſee how far we may depend upon this determination of Mars's diurnal rotation as coming near the truth; and looking over thoſe cauſes which may poſſibly produce any errors, we find, firſt of all, that in the long biennial periods a miſtake in the number of revolutions would produce a conſiderable deviation from truth. Secondly, in the obſervations of a ſpot which moves ſo ſlow, we are alſo liable to ſome conſiderable miſtake in eſtimating the time when it comes to a certain place; and the more ſo, if that place is not the center. Laſtly, the time itſelf is liable to inaccuracy.

As to the firſt, it appears from the three monthly periods obſerved in the year 1779, when the proper allowances for the geocentric places are made, that the ſydereal revolution of Mars cannot well be leſs than 24 h. 39′ 5″, nor more than 24 h. 39′ 22″; but if we ſhould divide any one of the three biennial periods by a ſuppoſed number of revolutions, only one more or one leſs than we have done, the difference would be ſo conſiderable, that nothing but a miſtake in every one of the three monthly periods, of at leaſt one whole hour, could juſtify ſuch a ſuppoſition; and that ſuch a miſtake in the ſituation of a ſpot on Mars cannot have been made in thoſe obſervations, I think, is evident enough from the exactneſs with which they were made, and from their agreement with each other.

The ſecond cauſe of error, which is the uncertainty in aſſigning the exact time when a ſpot comes to the center, is of ſome force. But it ſeems to me highly probable, from the manner in which I have ſeen the ſpots on Mars paſs over the diſk of that planet, that there can hardly be ſo great an error as 10′ in an obſervation of any remarkable ſpot's coming to the center. However, not being willing to truſt more to the eye than I ought to do, I had recourſe to the following experiment. I drew ſeveral circles of one inch radius, taking care to make no viſible impreſſion of a center; and placed in each a fine point at the ſeveral diſtances of .0424, .0636, .0848, in ten thouſands of an inch from the real center; ſome to the right, others to the left. Theſe meaſures are the ſines to radius one, of 2° 26′, 3° 39′, and 4° 52′, which are the arches a ſpot on Mars paſſes over in 10, 15, 20′ minutes reſpectively. I expoſed them to ſeveral perſons unacquainted with my deſigns, and found, that not one of them made a ſingle miſtake in ſaying whether the point was, or was not, in the center of the circle, and which way it deviated from it. As the direction of the motion of a ſpot on Mars is known, I thought the perſons who were to judge of the place of the points were intitled to be acquainted with the line in which they were placed, which for that reaſon was always to the right and left only. The points that anſwer to the excentricity of 15 and 20′ are indeed ſo viſibly out of the center, that I believe we may ſafely ſay, that any miſtake, in eſtimating the time of a ſpot on Mars coming to the center, cannot well exceed a quarter of an hour at the outſide.

As for the third and laſt occaſion of error, the time itſelf, I believe my manner of obtaining and keeping it in the year 1779 will appear ſatisfactory, and may, I think, be depended upon to a few ſeconds; but the obſervations of the year 1777, indeed, are far from having the ſame advantage. I was not then provided with an altitude inſtrument, therefore ſet my clock by a good ſun-dial, with the equation of time contained in the Nautical Almanac, and found it to agree generally to a minute or two with the time calculated for the eclipſes of Jupiter's firſt ſatellite, as I deduced it for Bath from the Nautical Almanac. However, it was certainly liable to an error of ſeveral minutes; therefore, allowing no leſs than 10′ for the clock in 1777, and 20′ for an error in eſtimating the ſituation of a ſpot in 1779, it will both amount to half an hour: then, if we take a mean of the three numbers, whereby we have divided the three biennial periods, we have $766\frac{1}{3}$; and half an hour, divided by $766\frac{1}{3}$, will therefore give us the quantity to which, it ſeems, can amount, all the uncertainty in the ſydereal diurnal rotation of Mars, which is 2″,34.

A nearer approximation to truth I hope to obtain at the next oppoſition, which will happen about the middle of July 1781.

I have ventured to calculate the times for that oppoſition, when the edge of the remarkable dark ſpot will be ſeen near the center, as it is in figure 23, or, which is the ſame thing, as it was the 11th of May 1779, at 12h. 17′. The ſpot not being viſible at the time of the oppoſition, I have taken the neareſt period, before and after, in which it will paſs over the diſk. There is, however, a circumſtance which may make the appearance of the ſpot not quite ſimilar to the figure I have drawn, even though the rotations ſhould perfectly anſwer as to the times; for the poſition of the axis of Mars being ſtill in ſome meaſure unknown, I could make no allowance for a change, which a difference in the ſituation of no leſs than two ſigns may occaſion, though in all probability it will not be very conſiderable.

Thoſe who are provided with proper teleſcopes will have an opportunity to ſee how far the calculated times agree with the ſpot's appearance; and it is by this means I alſo hope to correct and improve the tables I have drawn up for this purpoſe, and further to approximate to a true theory of the gyration of this planet.

Not knowing the exact difference of meridians between Greenwich and this place, I have calculated the ſpot's appearance for the meridian of Bath. From an eclipſe or two of Jupiter's ſatellites, of which, by the favour of the Rev. Mr. HORNSBY, I have ſeen correſpondent obſervations, I ſuppoſe the difference cannot be much leſs than 9′ weſt of Greenwich; and at the ſame time I join an account of the ſolar eclipſe of the 24th of June 1778, which may be depended upon as a very compleat obſervation, and may ſerve to aſcertain the longitude of this place.

Eclipſe of the ſun obſerved at Bath.

June 24, 1778.

	H.	M.	S.
Beginning by equated or mean time,	3	30	10,7
End, - - -	5	18	7,7

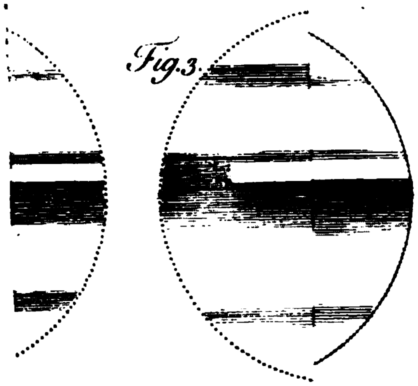
Calculations; or (as the principles on which they are founded are ſtill eſtabliſhed upon a few obſervations only, and require ſome time for mature confirmation) I would rather, if I might be allowed the expreſſion, call them calculated conjectures of the times when the remarkable dark ſpot will be ſeen near the center of the diſk of Mars.

For June, July, and Auguſt, of the year 1781.

	D.	H.	M.	S.
June	28	9	48	39
	29	10	27	10
	30	11	5	42
July	1	11	44	13
	2	12	22	32
	3	13	0	51
	4	13	39	10
	5	14	17	29
	6	14	55	46
	7	15	34	4
July	8	16	13	23
Auguſt	3	8	11	6
	4	8	49	57
	5	9	28	48
	6	10	7	39
	7	10	46	31
	8	11	25	44
	9	12	4	56
	10	12	44	8

I have the honour to be, &c.

Fig. 3.



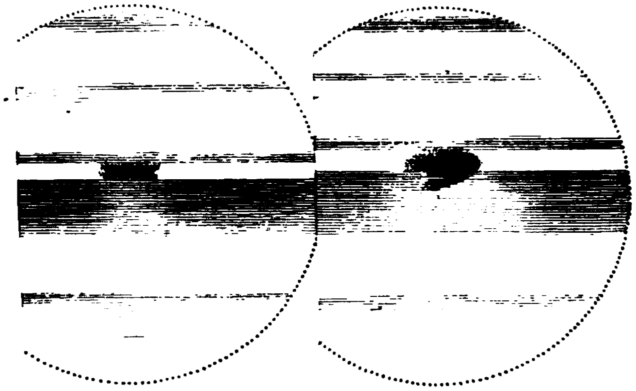
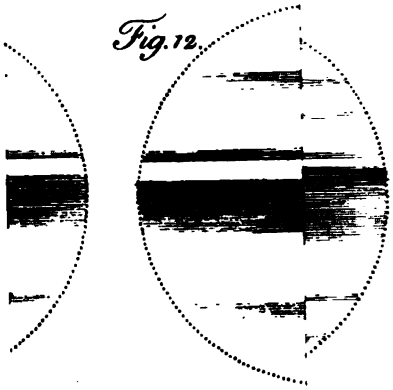


Fig. 12.



BASIRE SC.

Fig. 16.

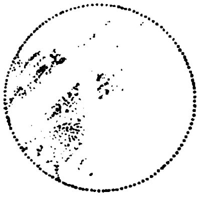


Fig. 24.

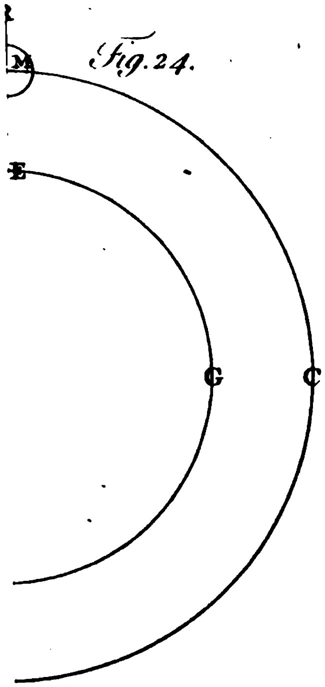


Fig. 19.

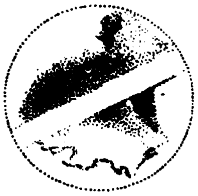
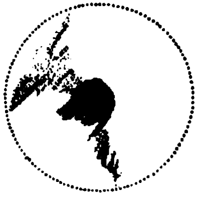


Fig. 22.



BASIRE SC.

XI. *Some Account of the* Termites, *which are found in* Africa *and other hot Climates. In a Letter from Mr.* Henry Smeathman, *of* Clement's Inn, *to Sir* Joſeph Banks, *Bart. P. R. S.*

Read February 15, 1781.

SIR,

Clement's Inn,
Jan. 23, 1781.

OF a great many curious parts of the creation I met with on my travels in that almoſt unknown diſtrict of Africa called Guinea, the TERMITES, which by moſt travellers have been called WHITE ANTS, ſeemed to me on many accounts moſt worthy of that exact and minute attention which I have beſtowed upon them.

The amazingly great and ſudden miſchief they frequently do to the property of people in tropical climates, makes them well known and greatly feared by the inhabitants.

The ſize and figure of their buildings have attracted the notice of many travellers, and yet the world has not hitherto been furniſhed with a tolerable deſcription of them, though their contrivance and execution ſcarce fall ſhort of human ingenuity and prudence; but when we come to conſider the wonderful oeconomy of theſe inſects, with the good order of their ſubterraneous cities, they will appear foremoſt on the liſt of the wonders of the creation, as moſt cloſely imitating mankind in provident induſtry and regular government.

You had barely time to ſee and to admire ſome of their buildings in New Holland, and have been pleaſed to ſay, you think an accurate account of them would meet a favourable reception from the Royal Society. That which I now have the honour to preſent to you, is accurate and faithful as far as it goes. I have kept as cloſe to my ſubject as was in my power, without being obſcure, or falling ſhort of my intention; and though I have given only the heads of what I could draw from my memorandums on the ſubject, they will probably be found ſufficiently deſcriptive and hiſtorical for the bounds of a letter.

The ſagacity of theſe little inſects is ſo infinitely beyond that of any other animals I have ever heard of, that it is poſſible the accounts I have here communicated would not appear credible to many, without ſuch vouchers and ſuch corroborating teſtimony as I am fortunately able to produce, and are now before you. There are alſo many living witneſſes in England to moſt of the extraordinary relations that I have given, ſo that I hope to have full credit for ſuch remarks as no one but myſelf has probably had time and opportunities enough to make, and which are not ſuſceptible of demonſtration, except in thoſe places where the inſects are found.

Such as they are, I beg leave to lay them, with all diffidence and humility, before you and that illuſtrious Body of which you are Preſident; and if they ſhould in a ſmall degree meet with approbation, I ſhall be exceedingly ſatisfied.

Theſe inſects are known by various names. They belong to the TERMES of LINNÆUS, and other ſyſtematical naturaliſts.

By the Engliſh,
In the windward parts of Africa they are called *Bugga Bugs*.
In the Weſt Indies, *Wood Lice*, *Wood Ants*, or *White Ants*.

By the French, { At Senegal, *Vague-Vagues.*
In the Weſt Indies, *Poux de Bois*, or *Fourmis Blanches.*

By the Bolms, or Sherbro people, in Africa, *Scantz.*

By the Portugueze in the Brazils, *Coupée* or *Cutters*, from their cutting things in pieces.

By this latter name and that of *Piercers* or *Eaters*, and ſimilar terms, they are diſtinguiſhed in various parts of the tropical regions.

The following are the ſpecific differences, given by Dr. SOLANDER, of ſuch inſects of this *genus* as I have obſerved and collected.

1. TERMES *bellicoſus* corpore fuſco, alis fuſceſcentibus: coſtâ ferrugineâ, ſtemmatibus ſubſuperis oculo propinquis, puncto centrali prominulo.
2. TERMES *mordax* nigricans, antennis pedibuſque teſtaceis, alis fuliginoſis: areâ marginali dilatatâ: coſtâ nigricante, ſtemmatibus inferis oculo approximatis, puncto centrali impreſſo.
3. TERMES *atrox* nigricans, ſegmentis abdominalibus margine pallidis, antennis pedibuſque teſtaceis, alis fuliginoſis: coſtâ nigrâ, ſtemmatibus inferis, puncto centrali impreſſo.
4. TERMES *deſtructor* nigricans, abdominis lineâ laterali luteâ, antennis teſtaceis, alis hyalinis: coſtâ luteſcente, ſtemmatibus ſubſuperis, puncto centrali obliterato.
5. TERMES *arborum* corpore teſtaceo, alis fuſceſcentibus: coſtâ luteſcente, capite nigricante, ſtemmatibus inferis oculo approximatis, puncto centrali impreſſo.

The Termites are repreſented by LINNÆUS as the greateſt plagues of both Indies, and are indeed every way between the

Tropics ſo deemed, from the vaſt damages they cauſe, and the loſſes which are experienced in conſequence of their eating and perforating wooden buildings, utenſils, and furniture, with all kinds of houſehold-ſtuff and merchandize, which are totally deſtroyed by them, if not timely prevented; for nothing leſs hard than metal or ſtone can eſcape their moſt deſtructive jaws.

They have been taken notice of by various travellers in different parts of the torrid zone; and indeed where numerous, as is the caſe in all equinoctial countries and iſlands that are not fully cultivated, if a perſon has not been incited by curioſity to obſerve them, he muſt have been very fortunate who, after a ſhort reſidence, has not been compelled to it for the ſafety of his property.

Theſe inſects have generally obtained the name of Ants, it may be preſumed, from the ſimilarity in their manner of living, which is, in large communities that erect very extraordinary neſts, for the moſt part on the ſurface of the ground, from whence their excurſions are made through ſubterraneous paſſages or covered galleries, which they build whenever neceſſity obliges, or plunder induces, them to march above ground, and at a great diſtance from their habitations carry on a buſineſs of depredation and deſtruction, ſcarce credible but to thoſe who have ſeen it. But notwithſtanding they live in communities, and are like the ants omnivorous; though like them at a certain period they are furniſhed with four wings, and emigrate or colonize at the ſame ſeaſon; they are by no means the ſame kind of inſects, nor does their form correſpond with that of *Ants* in any one ſtate of their exiſtence, which, like moſt other inſects, is changed ſeveral times.

The Termites reſemble the Ants alſo in their provident and diligent labour, but ſurpaſs them as well as the Bees, Waſps,

Beavers,

Beavers, and all other animals which I have ever heard of, in the arts of building, as much as the Europeans excel the leaſt cultivated ſavages. It is more than probable they excel them as much in ſagacity and the arts of government; it is certain they ſhew more ſubſtantial inſtances of their ingenuity and induſtry than any other animals; and do in fact lay up vaſt magazines of proviſions and other ſtores; a degree of prudence which has of late years been denied, perhaps without reaſon, to the Ants (1).

Such however are the extraordinary circumſtances attending their oeconomy and ſagacity, that it is difficult to determine, whether they are more worthy of the attention of the curious and intelligent part of mankind on theſe accounts, or from the ruinous conſequences of their depredations, which have deſervedly procured them the name of *Fatalis* or *Deſtructor*.

As this is the caſe, it is a little ſurpriſing that an accurate natural hiſtory of theſe wonderful inſects has not been attempted long ſince; eſpecially as, according to BOSMAN (who wrote the beginning of this century) in his deſcription of the Coaſt of Guinea, ſome curious cicumſtances relative to them muſt have been known. According to that gentleman, the *King* was ſuppoſed to be as large as a Cray-fiſh (2). This, though a bad compariſon, is pretty near the truth in reſpect to the ſize of the female, who is the *Common Mother* of the community;

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(1) Though Ants have no occaſion to lay up ſtores for winter in cold climates, they certainly muſt and do carry great quantities of proviſions into their neſts to feed the young brood; and moſt probably provide ſome before hand for fear of accidents, which might be fatal to the young ones, who, like all inſects in the caterpillar ſtate, are very voracious, and cannot bear diſappointments of long duration.

(2) BOSMAN'S Guinea, p. 260.

and, according to the mode we have adopted from time immemorial in ſpeaking of Ants and Bees, the QUEEN.

Theſe communities conſiſt of one *male* and one *female* (who are generally the *common parents* of the whole, or greater part, of the reſt), and of three orders of inſects, apparently of very different ſpecies, but really the ſame, which together compoſe great commonwealths, or rather monarchies, if I may be allowed the term.

The great LINNÆUS, having ſeen or heard of but two of theſe orders, has claſſed the genus erroneouſly; for he has placed it among the *Aptera*, or inſects without wings; whereas the chief order, that is to ſay, the inſect in its perfect ſtate, having four wings without any ſting, it belongs to the *Neuroptera*; in which claſs it will conſtitute a new genus of many ſpecies(3).

The different ſpecies of this genus reſemble each other in form, in their manner of living, and in their good and bad qualities: but differ as much as birds in the manner of building their habitations or neſts, and in the choice of the materials of which they compoſe them.

There are ſome ſpecies which build upon the ſurface of the ground, or part above and part beneath, and one or two ſpecies, perhaps more, that build on the ſtems or branches of trees, ſometimes aloft at a vaſt height.

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(3) I have no doubt, from the account and figures given of the European Termes Pulſatorius, or Death Watch, by the illuſtriqus BARON DE GEER, in his ſeventh volume of *Memoires pour ſervir à l' Hiſtoire des Inſectes*, that in their perfect ſtate they have wings, and ſwarm or emigrate, and live in a manner analogous to thoſe of hot climates; for they ſeem to have quite the external form of the exotic Termes, that is to ſay, of the firſt and third ordor. DE GEER, Memoires, tom. VII. p. 45. pl. IV. fig. 1, 2, 3, & 4.

Of every ſpecies there are three orders; firſt, the working inſects, which, for brevity, I ſhall generally call *labourers*; next the fighting ones, or *ſoldiers*, which do no kind of labour; and, laſt of all, the winged ones, or *perfect inſects*, which are male and female, and capable of propagation. Theſe might very appoſitely be called the *nobility* or *gentry*, for they neither labour, or toil, or fight, being quite incapable of either, and almoſt of ſelf-defence. Theſe only are capable of being elected kings or queens; and nature has ſo ordered it, that they emigrate within a few weeks after they are elevated to this ſtate, and either eſtabliſh new kingdoms, or periſh within a day or two.

The *Termes bellicoſus* being the largeſt ſpecies is moſt remarkable and beſt known on the Coaſt of Africa. It erects immenſe buildings of well-tempered clay or earth, which are contrived and finiſhed with ſuch art and ingenuity, that we are at a loſs to ſay, whether they are moſt to be admired on that account, or for their enormous magnitude and ſolidity. It is from the two lower orders of this, or a ſimilar ſpecies, that LINNÆUS ſeems to have taken his deſcription of the *Termes Fatalis*; and moſt of the accounts brought home from Africa or Aſia of the white Ants are alſo taken from a ſpecies that are ſo much alike in external habit and ſize, and build ſo much in their manner, that one may almoſt venture to pronounce them mere variations of the ſame ſpecies.

The reaſon that the larger Termites have been moſt remarked is obvious; they not only build larger and more curious neſts, but are alſo more numerous, and do infinitely more miſchief to mankind. When theſe inſects attack ſuch things as we would not wiſh to have injured, we muſt conſider them as moſt pernicious; but when they are employed in deſtroying decayed trees

and

and ſubſtances which only incumber the ſurface of the earth, they may be juſtly ſuppoſed very uſeful, and for the reaſon tha they are in one ſenſe moſt pernicious, they are in the other moſt uſeful. In this reſpect they reſemble very much the common Flies, which are regarded by mankind in general as noxious, and at beſt as uſeleſs beings in the creation; but this is certainly for want of conſideration. There are not probably in all nature animals of more importance, and it would not be difficult to prove, that we ſhould feel the want of one or two ſpecies of large quadrupeds, much leſs than of one or two ſpecies of theſe deſpicable-looking inſects. Mankind in general are ſenſible that nothing is more diſagreeable, or more peſtiferous, than putrid ſubſtances; and it is apparent to all who have made obſervation, that thoſe little inſects contribute more to the quick diſſolution and diſperſion of putreſcent matter than any other. They are ſo neceſſary in all hot climates, that even in the open fields a dead animal or ſmall putrid ſubſtance cannot be laid upon the ground two minutes before it will be covered with Flies and their Maggots, which inſtantly entering quickly devour one part, and perforating the reſt in various directions, expoſe the whole to be much ſooner diſſipated by the elements. Thus it is with the Termites; the rapid vegetation in hot climates, of which no idea can be formed by any thing to be ſeen in this, is equalled by as great a degree of deſtruction from natural as well as accidental cauſes (4). It ſeems apparent, that when any thing whatever is arrived at its laſt degree of perfection, the Creator has decreed it ſhall be totally deſtroyed as ſoon as poſſible, that the face of nature may be ſpeedily adorned with freſh productions in the bloom of ſpring or the pride of ſummer: ſo when trees, and even woods, are in part deſtroyed by tornadoes or fire, it is wonderful to obſerve, how many agents are employed in haſtening the total diſſolution of the reſt (5); but in the hot climates there are none ſo expert, or who do their buſineſs ſo expeditiouſly and effectually, as theſe inſects, who in a few weeks deſtroy and carry away the bodies of large trees, without leaving a particle behind, thus clearing the place for other vegetables, which ſoon fill up every vacancy; and in places, where two or three years before there has been a populous town, if the inhabitants, as is frequently the caſe, have choſen to abandon it, there ſhall be a very thick wood, and not the veſtige of a poſt to be ſeen, unleſs the wood has been of a ſpecies which, from its hardneſs, is called *iron wood.*

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(4) The Guinea graſs, which is ſo well known and ſo much eſteemed by our planters in the Weſt Indies, grows in Africa thirteen feet high upon an average, which height it attains in about five or ſix months; and the growth of many other plants is as quick.

My general account of the Termites is taken from obſervations made on the *Termes bellicoſus*, to which I was induced by the greater facility and certainty with which they could be made.

The neſts of this ſpecies are ſo numerous all over the iſland of Bananas, and the adjacent continent of Africa, that it is ſcarce poſſible to ſtand upon any open place, ſuch as a rice plantation, or other clear ſpot, where one of theſe buildings is not be ſeen within fifty paces, and frequently two or three are to be ſeen almoſt cloſe to each other. In ſome parts near Senegal, as mentioned by Monſ. ADANSON, their number, magnitude, and cloſeneſs of ſituation, make them appear like the villages of the natives (6): and you have yourſelf ſeen them perhaps ſtill more numerous, though not ſo large, in New Holland.

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(5) See STILLINGFLEET's Tracts.

(6) " But of all the extraordinary things I obſerved, nothing ſtruck me more " than certain eminences, which, by their height and regularity, made me take " them

Theſe buildings are uſually termed hills, by natives as well as ſtrangers, from their outward appearance, which is that of little hills more or leſs conical, generally pretty much in the form of ſugar loaves, and about ten or twelve feet in perpendicular height above the common ſurface of the ground (7) (8) (9), tab. VII. fig. 1.

Theſe

" them at a diſtance for an aſſemblage of negroes huts or a conſiderable village, " and yet they were only the neſts of certain inſects. They are round pyramids " from eight to ten feet high, upon nearly the ſame baſe, with a ſmooth ſurface " of rich clay, exceſſively hard and well built." ADANSON's Voyage to Senegal, 8vo, p. 153—337. Voyage de Senegal, 4to; p. 83 and 99.

Note. What Mr. ADANSON ſays of the opening which gives ingreſs and regreſs is manifeſtly a miſtake, ariſing from the natural concluſion that thoſe inſects had ſome way out and in to their neſts, without examining where it was. It will appear by this account, that they have many thouſand ways out and in, but all ſubterraneous.

(7) JOBSON, in his Hiſtory of Gambia, ſays, " The Ant hills are remarkable " caſt up in thoſe parts by Piſmires, ſome of them twenty foot in height, of " compaſſe to contayne a dozen men, with the heat of the ſun baked into that " hardneſſe, that we uſed to hide ourſelves in the ragged toppes of them, when " we took up ſtands to ſhoot at deere or wild beaſts." PURCHAS's Pilgrims, vol. II. p. 1570.

(8) " The Ants make neſts of the earth about *twice the height of a man.*" BOSMAN's Deſcription of Guinea, p, 276—493.

(9) The labourers are not quite a quarter of an inch in length; however, for the ſake of avoiding fractions, and of comparing them and their buildings with thoſe of mankind more eaſily, I eſtimate their length or height ſo much, and the human ſtandard of length or height, alſo to avoid fractions, at ſix feet, which is likewiſe above the height of men. If then one labourer is = to one-fourth of an inch = to ſix feet, four labourers are = to one inch in height = 24 feet, which multiplied by 12 inches, gives the comparative height of a foot of their building = 288 feet of the building of men, which multiplied by 10 feet, the ſuppoſed average height of one of their neſts is = 2880 of our feet, which is 240 feet more than half a mile, or near five times the height of the great pyramid; and, as it

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Theſe hills continue quite bare until they are ſix or eight feet high; but in time the dead barren clay, of which they are compoſed, becomes fertilized by the genial power of the elements in theſe prolific climates, and the addition of vegetable ſalts and other matters brought by the wind; and in the ſecond or third year, the hillock, if not over-ſhaded by trees, becomes, like the reſt of the earth, almoſt covered with graſs and other plants; and in the dry ſeaſon, when the herbage is burnt up by the rays of the ſun, it is not much unlike a very large hay-cock (10).

Every one of theſe buildings conſiſts of two diſtinct parts, the exterior and the interior.

The exterior is one large ſhell in the manner of a dome, large and ſtrong enough to incloſe and ſhelter the interior from the viciſſitudes of the weather, and the inhabitants from the attacks of natural or accidental enemies. It is always, therefore, much ſtronger than the interior building, which is the habitable part divided with a wonderful kind of regularity and contrivance into an amazing number of apartments for the reſidence of the *king* and *queen*, and the nurſing of their numerous

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is proportionably wide at the baſe, a great many times its ſolid contents. If to this compariſon we join that of the time in which the different buildings are erected, and conſider the Termites as raiſing theirs in the courſe of three or four years, the immenſity of their works ſets the boaſted magnitude of the antient wonders of the world in a moſt diminutive point of view, and gives a ſpecimen of induſtry and enterprize as much beyond the pride and ambition of men as St. Paul's Cathedral exceeds an Indian hut.

(10) See a figure of one of thoſe neſts in SALMON's Univerſal Traveller, in the map of Gambia, where it is called a Piſmire Hill: there is alſo a figure of one of the labouring inſects; but as the hill is repreſented below all proportion, and the inſect rather larger than life, it gives no idea of the building. I have not been able to find out from what author SALMON took this figure; and it is the only one I have met with.

merous progeny; or for magazines, which are always found well filled with ſtores and proviſions.

I ſhall forbear at this time entering into a very minute account of the inſide of theſe wonderful buildings, as the bare recital might appear tedious; though I flatter myſelf, that when I have an opportunity of communicating it to the publick at large, the readers will follow me through an exact deſcription of them with pleaſure.

Theſe hills make their firſt appearance above ground by a little turret or two in the ſhape of ſugar loaves, which are run a foot high or more (11). Soon after, at ſome little diſtance, while the former are increaſing in height and ſize, they raiſe others, and ſo go on increaſing the number and widening them at the baſe, till their works below are covered with theſe turrets, which they always raiſe the higheſt and largeſt in the middle, and by filling up the intervals, between each turret, collect them as it were into one dome.

They are not very curious or exact about theſe turrets, except in making them very ſolid and ſtrong, and when by the junction of them the dome is compleated, for which purpoſe the turrets anſwer as ſcaffolds, they take away the middle ones entirely, except the tops (which joined together make the crown of the cupola) and apply the clay to the building of the works within, or to erecting freſh turrets for the purpoſe of raiſing the hillock ſtill higher; ſo that no doubt ſome part of the clay is uſed ſeveral times, like the boards and poſts of a maſon's ſcaffold.

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(11) Some of theſe turrets are repreſented in the view of their hills, (tab. VII. fig. 3.). I have ſeen turrets on the ſides of theſe neſts four or five feet high (tab. VII. fig. 1. a. a. a.).

When theſe hills are at about little more than half their height, it is always the practice of the wild bulls to ſtand as centinels upon them, while the reſt of the herd is ruminating below (tab. VII.). They are ſufficiently ſtrong for that purpoſe, and at their full height anſwer excellently as places to look out. I have been with four men on the top of one of theſe hillocks. Whenever word was brought us of a veſſel in ſight, we immediately ran to ſome Bugga Bug hill, as they are called, and clambered up to get a good view, for upon the common ſurface it was ſeldom poſſible to ſee over the graſs or plants, which, in ſpite of monthly bruſhings, generally prevented all horizontal views at any diſtance.

The outward ſhell or dome is not only of uſe to protect and ſupport the interior buildings from external violence and the heavy rains; but to collect and preſerve a regular degree of genial warmth and moiſture which ſeems very neceſſary for hatching the eggs and cheriſhing the young ones.

The *royal chamber*, which I call ſo on account of its being adapted for, and occupied by, the *king* and *queen*, appears to be in the opinion of this little people of the moſt conſequence, being always ſituated as near the center of the interior building as poſſible, and generally about the height of the common ſurface of the ground, at a pace or two from the hillock. It is always nearly in the ſhape of half an egg or an obtuſe oval within, and may be ſuppoſed to repreſent a long oven (tab. VIII. fig. 1. and 2.).

In the infant ſtate of the colony, it is not above an inch or thereabout in length; but in time will be increaſed to ſix or eight inches or more in the clear, being always in proportion to the ſize of the *queen*, who, increaſing in bulk as in age, at length requires a chamber of ſuch dimenſions.

This ſingular part would bear a long deſcription, which I ſhall not trouble you with at preſent, and only obſerve, that its floor is perfectly horizontal; and in large hillocks, ſometimes an inch thick and upward of ſolid clay. The roof alſo, which is one ſolid and well-turned oval arch, is generally of about the ſame ſolidity, but in ſome places it is not a quarter of an inch thick, this is on the ſides where it joins the floor (tab. VIII. fig. 1. a. a.), and where the doors or entrances are made level therewith at pretty equal diſtances from each other (tab. VIII. fig. 2. and 4. b. b.)

Theſe entrances will not admit any animal larger than the ſoldiers or labourers, ſo that the *king*, and the *queen* (who is, at full ſize, a thouſand times the weight of a *king)* can never poſſibly go out.

The royal chamber, if in a large hillock, is ſurrounded by an innumerable quantity of others of different ſizes, ſhapes, and dimenſions; but all of them arched in one way or another, ſometimes circular, and ſometimes elliptical or oval.

Theſe either open into each other or communicate by paſſages as wide, and being always empty are evidently made for the ſoldiers and attendants, of whom it will ſoon appear great numbers are neceſſary, and of courſe always in waiting.

Theſe apartments are joined by the magazines and nurſeries. The former are chambers of clay, and are always well filled with proviſions, which to the naked eye ſeem to conſiſt of the raſpings of wood and plants which the Termites deſtroy, but are found in the microſcope to be principally the gums or inſpiſſated juices of plants. Theſe are thrown together in little maſſes, ſome of which are finer than others, and reſemble the ſugar about preſerved fruits, others are like tears of gum, one quite tranſparent, another like amber, a third brown, and a fourth quite opaque, as we ſee often in parcels of ordinary gums.

Theſe magazines are intermixed with the nurſeries, which are buildings totally different from the reſt of the apartments: for theſe are compoſed entirely of wooden materials, ſeemingly joined together with gums. I call them the nurſeries becauſe they are invariably occupied by the eggs, and young ones, which appear at firſt in the ſhape of labourers, but white as ſnow. Theſe buildings are exceeding compact; and divided into many very ſmall irregular-ſhaped chambers, not one of which is to be found of half an inch in width (tab. VIII. fig. 5.). They are placed all round the royal apartments, and as near as poſſible to them.

When the neſt is in the infant ſtate, the nurſeries are cloſe to the royal chamber; but as in proceſs of time the queen enlarges, it is neceſſary to enlarge the chamber for her accommodation; and as ſhe then lays a greater number of eggs, and requires a greater number of attendants, ſo it is neceſſary to enlarge and encreaſe the number of the adjacent apartments; for which purpoſe the ſmall nurſeries which are firſt built are taken to pieces, rebuilt a little farther off a ſize bigger, and the number of them encreaſed at the ſame time.

Thus they continually enlarge their apartments, pull down, repair, or rebuild, according to their wants, with a degree of ſagacity, regularity, and foreſight, not even imitated by any other kind of animals or inſects that I have yet heard of.

There is one remarkable circumſtance attending the nurſeries, which I muſt not at this time omit. They are always found ſlightly overgrown with *mould* (tab. VIII. fig. 6), and plentifully ſprinkled with ſmall white globules about the ſize of a ſmall pin's head. Theſe at firſt I took to be the eggs; but, on bringing them to the microſcope, they evidently appeared to be a ſpecies of muſhroom, in ſhape like our eatable muſhroom in the young ſtate in which it is pickled (tab. VIII. fig. 7.). They appear, when whole, white like ſnow a little thawed and then frozen again, and when bruiſed ſeem compoſed of an infinite number of pellucid particles, approaching to oval forms, and difficult to ſeparate; the mouldineſs ſeems likewiſe to be the ſame kind of ſubſtance (12).

The nurſeries are incloſed in chambers of clay, like thoſe which contain the proviſions, but much larger. In the early ſtate of the neſt they are not bigger than an hazel-nut, but in great hills are often as large as a child's head of a year old.

The diſpoſition of the interior parts of theſe hills is pretty much alike, except when ſome inſurmountable obſtacle prevents; for inſtance, when the *king* and *queen* have been firſt lodged near the foot of a rock or of a tree, they are certainly built out of the uſual form, otherwiſe pretty nearly according to the following plan.

The royal chamber is ſituated at about a level with the ſurface of the ground, at an equal diſtance from all the ſides of the building, and directly under the apex of the hill (tab. VII. fig. 2. A. A).

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(12) Mr KONIG, who has examined theſe kind of neſts in the Eaſt Indies, in an Eſſay upon the Termites, read before the Society of Naturaliſts of Berlin, conjectures, that theſe muſhrooms are the food of the young inſects. This ſuppoſition implies, that the old ones have a method of providing for and promoting their growth; a circumſtance which, however ſtrange to thoſe unacquainted with the ſagacity of theſe Inſects, I will venture to ſay, from many other extraordinary facts I have ſeen of them, is not very improbable.

N. B. Mr. KONIG has not diſcovered the magazines of proviſions in the neſts which he opened, as far as I am informed; but I muſt obſerve here, that what I have learned of this gentleman's account was from an extempore tranſlation of the heads of it.

It is on all ſides, both above and below, ſurrounded by what I ſhould call the *royal apartments*, which have only labourers and ſoldiers in them, and can be intended for no other purpoſe than for theſe to wait in, either to guard or ſerve their common FATHER and MOTHER, on whoſe ſafety depends the happineſs, and, according to the negroes, even the exiſtence of the whole community.

Theſe apartments compoſe an intricate labyrinth, which extends a foot or more in diameter from the *royal chamber* on every ſide. Here the nurſeries and magazines of proviſions begin, and, being ſeparated by ſmall empty chambers and galleries, which go round them or communicate from one to the other, are continued on all ſides to the outward ſhell, and reach up within it two-thirds or three-fourths of its height, leaving an open area in the middle under the dome, which very much reſembles the nave of an old cathedral: this is ſurrounded by three or four very large Gothic-ſhaped arches, which are ſometimes two or three feet high next the front of the area, but diminiſh very rapidly as they recede from thence like the arches of aiſles in perſpectives, and are ſoon loſt among the innumerable chambers and nurſeries behind them.

All theſe chambers, and the paſſages leading to and from them, being arched, they help to ſupport one another; and while the interior large arches prevent them falling into the center, and keep the area open, the exterior building ſupports them on the outſide.

There are, comparatively ſpeaking, few openings into the great area, and they for the moſt part ſeem intended only to admit that genial warmth into the nurſeries which the dome collects.

The interior building or aſſemblage of nurſeries, chambers, &c. has a flattiſh top or roof without any perforation, which would keep the apartments below dry, in caſe through accident the dome ſhould receive any injury and let in water; and it is never exactly flat and uniform, becauſe they are always adding to it by building more chambers and nurſeries: ſo that the diviſions or columns between the future arched apartments reſemble the pinnacles upon the fronts of ſome old buildings, and demand particular notice as affording one proof that for the moſt part the inſects project their arches, and do not make them, as I imagined for a long time, by excavation (tab. VII. fig. 2. B).

The area has alſo a flattiſh floor, which lays over the royal chamber, but ſometimes a good height above it, having nurſeries and magazines between (tab. VII. fig. 2. C.). It is likewiſe water-proof, and contrived, as far as I could gueſs, to let the water off, if it ſhould get in, and run over by ſome ſhort way into the ſubterraneous paſſages which run under the loweſt apartments in the hill in various directions, and are of an aſtoniſhing ſize, being wider than the bore of a great cannon. I have a memorandum of one I meaſured, perfectly cylindrical, and thirteen inches in diameter (tab. VII. fig. 2. D. D.).

Theſe ſubterraneous paſſages or galleries are lined very thick with the ſame kind of clay of which the hill is compoſed, and aſcend the inſide of the outward ſhell in a ſpiral manner, and winding round the whole building up to the top interſect each other at different heights, opening either immediately into the dome in various places, and into the interior building, the new turrets, &c. or communicating thereto by other galleries of different bores or diameters, either circular or oval.

From every part of theſe large galleries are various ſmall pipes or galleries leading to different parts of the building.

Under ground there are a great many which lead downward by ſloping deſcents three and four feet perpendicular among the gravel, from whence the labouring Termites cull the finer parts, which, being worked up in their mouths to the conſiſtence of mortar, becomes that ſolid clay or ſtone of which their hills and all their buildings, except their nurſeries, are compoſed.

Other galleries again aſcend and lead out horizontally on every ſide, and are carried under ground near to the ſurface a vaſt diſtance: for if you deſtroy all the neſts within one hundred yards of your houſe, the inhabitants of thoſe which are left unmoleſted farther off will nevertheleſs carry on their ſubterraneous galleries, and invade the goods and merchandizes contained in it by ſap and mine, and do great miſchief, if you are not very circumſpect.

But to return to the cities from whence theſe extraordinary expeditions and operations originate, it ſeems there is a degree of neceſſity for the galleries under the hills being thus large, being the great thoroughfares for all the labourers and ſoldiers going forth or returning upon any buſineſs whatever, whether fetching clay, wood, water, or proviſions; and they are certainly well calculated for the purpoſes to which they are applied, by the ſpiral ſlope which is given them; for if they were perpendicular the labourers would not be able to carry on their building with ſo much facility, as they aſcend a perpendicular with great difficulty, and the ſoldiers can ſcarce do it at all. It is on this account that ſometimes a road like a ledge is made on the perpendicular ſide of any part of the building within their hill, which is flat on the upper ſurface, and half an inch wide, and aſcends gradually like a ſtair-caſe, or like thoſe roads which are cut on the ſides of hills and mountains, that would otherwiſe be inacceſſible: by which, and ſimilar contrivances, they travel with great facility to every interior part.

This too is probably the cauſe of their building a kind of bridge of one vaſt arch, which anſwers the purpoſe of a flight of ſtairs from the floor of the area to ſome opening on the ſide of one of the columns which ſupport the great arches, which muſt ſhorten the diſtance exceedingly to thoſe labourers who have the eggs to carry from the royal chamber to ſome of the upper nurſeries, which in ſome hills would be four or five feet in the ſtraighteſt line, and much more if carried through all the winding paſſages which lead through the inner chambers and apartments.

I have a memorandum of one of theſe bridges, half an inch broad, a quarter of an inch thick, and ten inches long, making the ſide of an elliptic arch of proportionable ſize; ſo that it is wonderful it did not fall over or break by its own weight before they got it joined to the ſide of the column above. It was ſtrengthened by a ſmall arch at the bottom, and had a hollow or groove all the length of the upper ſurface, either made purpoſely for the inhabitants to travel over with more ſafety, or elſe, which is not improbable, worn ſo by frequent treading (tab. VII. fig. 2. E. E.).

Thus I have deſcribed, as briefly as the ſubject would admit, and I truſt without exaggeration, thoſe wonderful buildings whoſe ſize and external form have often been mentioned by travellers, but whoſe interior and more curious parts are ſo little known, that I may venture to conſider my account of them as new, which is the only merit it has: for they are conſtructed upon ſo different a plan from any thing elſe upon the earth, and ſo complicated, that I cannot find words equal to the taſk, and muſt therefore refer to the different figures, which, however extraordinary, ſcarce do juſtice to the ſubjects.

The neſts before deſcribed are ſo remarkable on account of their ſize, that travellers have ſeldom, where they were to be ſeen, taken notice of any other; and have generally, when ſpeaking of white Ants, deſcribed them as inhabitants of thoſe hills. Thoſe, however, which are built by the ſmaller ſpecies of thoſe inſects, are very numerous, and ſome of them exceedingly worth our attention; one ſort in particular, which from their form I have named turret neſts. Theſe are a great deal leſs than the foregoing, and indeed much leſs in proportion to the ſize of the builders; but their external form is more curious, and their ſolidity conſidered they are prodigious buildings for ſo ſmall an animal (13).

Theſe buildings are upright cylinders compoſed of a well-tempered black earth or clay, about three quarters of a yard high, and covered with a roof of the ſame material in the ſhape of a cone, whoſe baſe extends over and hangs down three or four inches wider than the perpendicular ſides of the cylinder, ſo that moſt of them reſemble in ſhape the body of a round wind-mill; but ſome of the roofs have ſo little elevation in the middle, that they are pretty much in the ſhape of the top of a full-grown muſhroom (tab. IX. fig. 1.)

After one of theſe turrets is finiſhed, it is not altered or enlarged; but when no longer capable of containing the community, the foundation of another is laid within a few inches of it. Sometimes, though but rarely, the ſecond is begun before the firſt is finiſhed, and a third before they have completed the ſecond: thus they will run up five or ſix of theſe turrets at the foot of a tree in the thick woods, and make a moſt ſingular group of buildings (tab. IX.).

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(13) If their height is eſtimated and computed by the ſize of the builders, and compared with ours upon the like ſcale; each of them is four or five times the height of the monument, and a great many times its ſolid contents.

The turrets are ſo ſtrongly built, that in caſe of violence they will much ſooner overſet from the foundation, and tear up the gravel and ſolid earth, than break in the middle; and in that caſe the inſects will frequently begin another turret and build it, as it were, through that which is fallen; for they will connect the cylinder below with the ground, and run up a new turret from its upper ſide, ſo that it will ſeem to reſt upon the horizontal cylinder only (tab. IX. fig. 5.).

I have not obſerved any thing elſe about theſe neſts that is remarkable, except the quality of the black brown clay, which is as dark coloured as rich vegetable mould, but burns to an exceeding fine and clear red brick. Within, the whole building is pretty equally divided into innumerable cells of irregular ſhapes; ſometimes they are quadrangular or cubic, and ſometimes pentagonal; but often the angles are ſo ill defined, that each half of a cell will be ſhaped like the inſide of that ſhell which is called the Sea-ear.

Each cell has two or more entrances, and as there are no pipes or galleries, no variety of apartments, no well-turned arches, wooden nurſeries, &c. &c. they do not by any means excite our admiration ſo much as the hill neſts, which are indeed collections of wonders.

There are two ſizes of theſe turret neſts, built by two different ſpecies of Termites. The larger ſpecies, the *Termes atrox*, in its perfect ſtate meaſures one inch and three-tenths from the extremities of the wings on the one ſide to the extremities on the other (tab. X. fig. 14.). The leſſer ſpecies, *Termes mordax*, meaſures only eight-tenths of an inch from tip to tip (tab. X. fig. 10.

The next kind of neſts, built by another ſpecies of this genus, the *Termes arborum,* have very little reſemblance to the former in ſhape or ſubſtance. Theſe are generally ſpherical or oval, and built in trees (14). Sometimes they are ſeated between the arms and the ſtems of trees, and very frequently may be ſeen ſurrounding the branch of a tree at the height of ſeventy or eighty feet; and (though but rarely of ſo large a ſize) as big as a very great ſugar caſk (15) (16).

They are compoſed of ſmall particles of wood and the various gums and juices of trees, combined with, perhaps, thoſe of the animals, and worked by thoſe little induſtrious creatures into a paſte, and ſo moulded into innumerable little cells of very different and irregular forms, which afford no amuſing variety and nothing curious, but the immenſe quantity of inhabitants, young and old, with which they are at all times crouded; on which account they are ſought for in order to feed young fowls, and eſpecially for the rearing of Turkies. Theſe neſts are very compact, and ſo ſtrongly attached to the boughs on which they are fixed, that there is no detaching them but by cutting them in pieces, or ſawing off the branch; and they will ſuſtain the force of a tornado as long as the tree on which they are fixed.

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(14) The colour of theſe neſts, like that of the roofed turrets, is black, from which, and their irregular ſurface and orbicular ſhape, they have been called *Negro Heads* by our firſt writers on the Carribbee Iſlands, and by the French, *Tetes des Negres.* See HUNTER's EVELYN's SILVA, p. 17.

I have never been able to diſcover what author Mr. EVELYN alludes to in this mention of the Negro Heads.

(15) LONG's Jamaica, vol. III. p. 887.

(16) SLOANE's Jamaica, vol. II. p. 221. and ſequel.

This ſpecies has the external habit, ſize, and almoſt the colour, of the *Termes atrox* (tab. X. fig. 21.).

There are ſome neſts built in thoſe ſandy plains which we call, after the Spaniards, *Savannas*, that reſemble the hill neſts firſt deſcribed. They are compoſed of a black mud, which is brought from a few inches below the white ſand, and are built in the form of an imperfect cone, or bell-ſhaped, having their tops rounded. Theſe neſts are generally about four or five feet high (17). As I ſaw theſe only in paſſing through various Savannahs upon other purſuits, I can ſay very little of their interior parts. They ſeemed to be inhabited by nearly as large inſects, differing very little except in colour, which is lighter than that of the *Termites bellicoſi*.

Having given ſome idea of the neſts, I ſhall beg your patient reading of a more particular account of the inſects themſelves, which will be exceeding neceſſary to a tolerable acquaintance with their oeconomy and management, their manner of building, fighting, and marching, and to a more particular account of their uſes in the creation, and of the vaſt miſchief they cauſe to mankind.

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(17) " The neſts of Ants are about four feet wide at the baſe, and two high, " of an hemiſpherical form. Though made in looſe ſand, they are ſo hard as " not to be broken without great efforts, and a laden cart could not break " through. — In October and November they add a new ſtory. — The Cochons " de Terre (the Leſt Ant-eater of Mr. PENNANT) make holes in theſe neſts eight " inches in diameter and ſix deep; *and having deſtroyed the inhabitants, the neſt* " *is abandoned, but ſometimes the Ants repair it.*" This laſt paragraph ſeems rather founded on conjecture. Voyage au Cap, par M. L'Abbé DE LA CAILLE, p. 305—356.

OVIEDO alſo ſays Ants make hillocks as high as a man.

Among theſe you will find, I muſt confeſs, ſome very extraordinary relations, and many that do not admit a poſſibility of demonſtration; ſuch is the deſcription of the form of an army of the *Termites viarum* marching, and the account of the regularity uſed by the *Termites bellicoſi* in repairing a breach in their hills. But the very ſingular facts, of which you have the proofs before you, are ſufficient I ſhould conceive to procure me belief for the others. Should any perſon doubt, I would wiſh them to conſider, that a ſtudent of nature and nature's laws, in any matter relating thereto, has no temptation to tranſgreſs the bounds of truth. I am very ſenſible, that the works of the creation, and the order thereof, are eſtabliſhed in the higheſt wiſdom; that it is as abſurd to attempt to exaggerate as to detract from them; and can only ſerve to expoſe the ignorance of him who attempts it. Beſides, what I have here advanced muſt be confirmed or contradicted in two or three years, ſince it will doubtleſs be examined into by all the curious who viſit tropical regions.

I have obſerved before, that there are of every ſpecies of *Termites* three orders; of theſe orders the working inſects or labourers are always the moſt numerous; in the *Termes bellicoſus* there ſeems to be at the leaſt one hundred labourers to one of the fighting inſects or ſoldiers. They are in this ſtate about one-fourth of an inch long, and twenty-five of them weigh about a grain; ſo that they are not ſo large as ſome of our ants (tab. X. fig. 6.). From their external habit and fondneſs for wood, they have been very expreſſively called *Wood Lice* by ſome people, and the whole genus has been known by that name, particularly among the French. They reſemble them, it is true, very much at a diſtance, but they run as faſt or faſter than any other inſects of their ſize, and are inceſſantly buſtling about their affairs (18).

The ſecond order, or ſoldiers, have a very different form from the labourers, and have been by ſome authors ſuppoſed to be the males, and the former neuters; but they are, in fact, the ſame inſects as the foregoing, only they have undergone a change of form, and approached one degree nearer to the perfect ſtate. They are now much larger, being half an inch long, and equal in bulk to fifteen of the labourers (tab. X. fig. 8).

There is now likewiſe a moſt remarkable circumſtance in the form of the head and mouth; for in the former ſtate the mouth is evidently calculated for gnawing and holding bodies; but in this ſtate, the jaws being ſhaped juſt like two very ſharp awls a little jagged (tab. X. fig 9.), they are incapable of any thing but piercing or wounding, for which purpoſes they are very effectual, being as hard as a crab's claw, and placed in a ſtrong horny head, which is of a nut-brown colour, and larger than all the reſt of the body together, which ſeems to labour under great difficulty in carrying it: on which account perhaps the animal is incapable of climbing up perpendicular ſurfaces.

The third order, or the inſect in its perfect ſtate, varies its form ſtill more than ever. The head, thorax, and abdomen, differ almoſt entirely from the ſame parts in the labourers and ſoldiers; and, beſides this, the animal is now furniſhed with four fine large browniſh, tranſparent, wings, with which it is at the time of emigration to wing its way in ſearch of a new ſettlement (19). In ſhort, it differs ſo much from its form and appearance in the other two ſtates, that it has never been ſuppoſed to be the ſame animal, but by thoſe who have ſeen it in the ſame neſt; and ſome of theſe have diſtruſted the evidence of their ſenſes. It was ſo long before I met with them in the neſts myſelf, that I doubted the information which was given me by the natives, that they belonged to the ſame family (tab. X. fig. 1.) Indeed we may open twenty neſts without finding one winged one, for thoſe are to be found only juſt before the commencement of the rainy ſeaſon, when they undergo the laſt change, which is preparative to their colonization. Add to this, they ſometimes abandon an outward part of their building, the community being diminiſhed by ſome accident to me unknown. Sometimes too different ſpecies of the real Ant (Formica) poſſeſs themſelves by force of a lodgement, and ſo are frequently diſlodged from the ſame neſt, and taken for the ſame kind of inſects. This I know is often the caſe with the neſts of the ſmaller ſpecies, which are frequently totally abandoned by the Termites, and completely inhabited by different ſpecies of Ants, Cockroaches, Scolopendræ, Scorpions, and other vermin, fond of obſcure retreats, that occupy different parts of their roomy buildings; which clearly accounts for your having met with the real Ants in thoſe neſts in New Holland.

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(18) ROCHFORT, in the Hiſtory of the Carribee Iſlands, calls them Wood Lice, and mentions the deſtruction they make, &c. p. 149.

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(19) " There is a ſort that frequently flies, having red wings.—This flying " ſort flings up the largeſt hills, and is wonderfully nimble and induſtrious." KOLBEIN's Cape of Good Hope, 8vo, vol. II. p. 173.

DAPPER calls the Wood Ants *Acolalan*, and ſays it becomes as big as one's thumb, and then takes wing. Deſcription de l'Afrique, folio, p. 459.

In the winged ſtate they have alſo much altered their ſize as well as form. Their bodies now meaſure between ſix and ſeven tenths of an inch in length, and their wings above two inches and a half from tip to tip, and they are equal in bulk to about thirty labourers, or two ſoldiers. They are now alſo furniſhed with two large eyes placed on each ſide of the head, and very conſpicuous; if they have any before, they are not eaſily to be diſtinguiſhed. Probably in the two firſt ſtates, their eyes, if they have any, may be ſmall like thoſe of moles; for as they live like theſe animals always underground, they have as little occaſion for theſe organs, and it is not to be wondered at that we do not diſcover them; but the caſe is much altered when they arrive at the winged ſtate in which they are to roam, though but for a few hours, through the wide air, and explore new and diſtant regions. In this form the animal comes abroad during or ſoon after the firſt tornado, which at the latter end of the dry ſeaſon proclaims the approach of the enſuing rains, and ſeldom waits for a ſecond or third ſhower, if the firſt, as is generally the caſe, happens in the night, and brings much wet after it (20).

The quantities that are to be found the next morning all over the ſurface of the earth, but particularly on the waters, is aſtoniſhing; for their wings are only calculated to carry them a few hours, and after the riſing of the ſun not one in a thouſand is to be found with four wings, unleſs the morning continues rainy, when here and there a ſolitary being is ſeen winging its way from one place to another, as if ſolicitous only to avoid its numerous enemies, particularly various ſpecies of Ants which are hunting on every ſpray, on every leaf, and in every poſſible place, for this unhappy race, of which probably not a pair in many millions get into a place of ſafety, fulfil the firſt law of nature, and lay the foundation of a new community.

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(20) "At night I viſited Mr. HARRISON on board the ſloop; during the time "we had a dreadful tornado, in which a ſort of large flies with long wings came "on board in ſuch prodigious numbers, that flying into the flames of the "candles, the table was ſoon covered with thoſe that burnt their wings; and "others, which were not burnt, as they walked along the table ſhed their wings, "and then were nothing but ſo many perfect large maggots." June 10, 1732. MOOR's Travels, p. 118.

Not only all kinds of ants, birds, and carnivorous reptiles, as well as inſects, are upon the hunt for them, but the inhabitants of many countries, and particularly of that part of Africa where I was, eat them (21) (22) (23) (24) (25).

On

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(21) Mr. KONIG, in an Eſſay upon theſe Inſects, read before the Society of Naturaliſts of Berlin, ſays, That, in ſome parts of the Eaſt Indies, the queens are given alive to old men for ſtrengthening the back, and that the natives have a method of catching the winged inſects, which he calls females, before the time of emigration. They make two holes in the neſt; the one to windward, and the other to leeward. At the leeward opening they place the mouth of a pot, previouſly rubbed within with an aromatic herb called *Bergera*, which is more valued there than the laurel in Europe. On the windward ſide they make a fire of ſtinking materials, which not only drives theſe inſects into the pots, but frequently the hooded ſnakes alſo, on which account they are obliged to be cautious in removing them. By this method they catch great quantities, of which they make with flour a variety of paſtry, which they can afford to ſell very cheap to the poorer ranks of people. Mr. KONIG adds, that in ſeaſons when this kind of food is very plentiful, the too great uſe of it brings on an epidemic colic and dyſentery, which kills in two or three hours.

I have

On the following morning, however, as I have obſerved, they are to be ſeen running upon the ground in chace of each other;

I have not found the Africans ſo ingenious in procuring or dreſſing them. They are content with a very ſmall part of thoſe which, at the time of ſwarming, or rather of emigration, fall into the neighbouring waters, which they ſkim off with calabaſhes, bring large kettles full of them to their habitations, and parch them in iron pots over a gentle fire, ſtirring them about as is uſually done in roaſting coffee. In that ſtate, without ſauce or any other addition, they ſerve them as delicious food; and they put them by hands-full into their mouths, as we do comfits. I have eat them dreſſed this way ſeveral times, and think them both delicate, nouriſhing, and wholeſome; they are ſomething ſweeter, but not ſo fat and cloying as the caterpillar or maggot of the *Palm-tree Snout-beetle*, *Curculio Palmarum*, which is ſerved up at all the luxurious tables of Weſt Indian epicures, particularly of the French, as the greateſt dainty of the Weſtern world.

According to the Baron DE GEER, Mr. SPARRMAN ſays, that the Hottentots eat theſe inſects, and even grow fat upon them; but does not ſay what methods they take to procure or dreſs them. DE GEER, *Memoires des Inſectes*, tom. VII. p. 49.

(22) PISO, DE LAET, MARCGRAVE, and other writers, mention their being an article of diet in different parts of South America.

" Alia præterea datur grandis ſpecies *Tama-ioura* dicta digiti articulum adæ- " quans. Quarum etiam clunes deſſecantur et friguntur pro bono alimento." PISO, Hiſt. Natural. lib. I. p. 9. lib. V. 291.

(23) MARCGR. Hiſt. Nat. 56.

(24) " Denique formicæ hic viſuntur grandiſſimæ, quas indigenæ vulgo come- " dunt; et in foris venales habent." DE LAET. Americæ Utriuſque Deſcriptio, p. 333.

" Formicis veſcebantur, eaſque ſtudioſe ad victum educabant. Ibid. p. 379."

(25) Sir HANS SLOANE ſays, the ſilk-cotton-tree worm is eſteemed by the Indians and negroes beyond marrow. This worm is no more than a large maggot, being the Caterpillar of a large Capricorn Beetle, or Goat Chafer: the Larva of a pretty large Cerambix (the *Lamia Tribulus* of FABRICIUS) which is alſo brought from Africa, where I have eaten thoſe worms roaſted. This inſect is moſt probably to be found in all countries where the ſilk-cotton-tree (*Bombax*) is indigenous. SLOANE's Jamaica, vol. II. p. 193.

other; ſometimes with one or two wings ſtill hanging to their bodies, which are not only uſeleſs, but ſeem rather cumberſome (26).

The greater part have no wings, but they run exceeding faſt, the males after the females; I have ſometimes remarked two males after one female, contending with great eagerneſs who ſhould win the prize, regardleſs of the innumerable dangers that ſurrounded them.

They are now become from one of the moſt active, induſtrious, and rapacious, from one of the moſt fierce and implacable little animals in the world, the moſt innocent, helpleſs, and cowardly; never making the leaſt reſiſtance to the ſmalleſt Ant. The Ants are to be ſeen on every ſide in infinite numbers, of various ſpecies and ſizes, dragging theſe annual victims of the laws of nature to their different neſts. It is wonderful that a pair ſhould ever eſcape ſo many dangers, and get into a place of ſecurity. Some, however, are ſo fortunate; and being found by ſome of the labouring inſects that are continually running about the ſurface of the ground under their covered galleries, which I ſhall ſhortly deſcribe, are *elected* KINGS and QUEENS of new ſtates; all thoſe who are not ſo elected and preſerved certainly periſh, and moſt probably in the courſe of the following day. The manner in which theſe labourers protect the happy pair from their innumerable enemies, not only on the day of the

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I have diſcourſed with ſeveral gentlemen upon the taſte of the white Ants; and on comparing notes we have always agreed, that they are moſt delicious and delicate eating. One gentleman compared them to ſugared marrow, another to ſugared cream and a paſte of ſweet almonds.

(26) LIGON obſerved them, but does not know what they are. LIGON's Barbadoes, p. 63.

maſſacre of almoſt all their race, but for a long time after, will I hope juſtify me in the uſe of the term ELECTION. The little induſtrious creatures immediately encloſe them in a ſmall chamber of clay ſuitable to their ſize, into which at firſt they leave but one ſmall entrance, large enough for themſelves and the ſoldiers to go in and out, but much too little for either of the royal pair to make uſe of; and when neceſſity obliges them to make more entrances, they are never larger; ſo that, of courſe, the *voluntary ſubjects* charge themſelves with the taſk of providing for the offspring of their ſovereigns as well as to work and to fight for them until they ſhall have raiſed a progeny capable at leaſt of dividing the taſk with them.

It is not until this time, probably, that they conſummate their marriage, as I never ſaw a pair of them joined. The buſineſs of propagation, however, ſoon commences, and the labourers having conſtructed a ſmall wooden nurſery, as before deſcribed, carry the eggs and lodge them there as faſt as they can obtain them from the *queen*.

About this time a moſt extraordinary change begins to take place in the *queen*, to which I know nothing ſimilar, except in the PULEX PENETRANS of LINNÆUS, the JIGGER of the *Weſt Indies*, and in the different ſpecies of COCCUS, COCHINEAL. The abdomen of this female begins gradually to extend and enlarge to ſuch an enormous ſize, that an *old queen* will have it increaſed ſo as to be *fifteen hundred* or *two thouſand times* the bulk of the reſt of her body, and *twenty or thirty thouſand times* the bulk of a labourer, as I have found by carefully weighing and computing the different ſtates (tab. X. fig. 3.). The ſkin between the ſegments of the abdomen extends in every direction; and at laſt the ſegments are removed to half an inch diſtance from each other, though at firſt the length of the whole abdomen is not half an inch. They preſerve their dark brown colour, and the upper part of the abdomen is marked with a regular ſeries of brown bars from the thorax to the poſterior part of the abdomen, while the intervals between them are covered with a thin, delicate, tranſparent ſkin, and appear of a fine cream colour, a little ſhaded by the dark colour of the inteſtines and watery fluid ſeen here and there beneath. I conjecture the animal is upward of two years old when the abdomen is increaſed to three inches in length: I have ſometimes found them of near twice that ſize. The abdomen is now of an irregular oblong ſhape, being contracted by the muſcles of every ſegment, and is become one vaſt matrix full of eggs, which make long circumvolutions through an innumerable quantity of very minute veſſels that circulate round the inſide in a ſerpentine manner, which would exerciſe the ingenuity of a ſkilful anatomiſt to diſſect and develope. This ſingular matrix is not more remarkable for its amazing extenſion and ſize than for its periſtaltic motion, which reſembles the undulating of waves, and continues inceſſantly without any apparent effort of the animal; ſo that one part or other alternately is riſing and ſinking in perpetual ſucceſſion, and the matrix ſeems never at reſt (27), but is always protruding eggs to the amount (as I have frequently counted in old queens) of ſixty in a minute (28), or eighty thouſand and upward in one day of twenty-four hours (29).

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(27) "We may obſerve in a *queen*, diſtended with egg, a partition along the "back, and a continued motion from one extreme to the other, much like "that we find in ſilk-worms." Account of Engliſh Ants by GOULD, p. 22.

(28) I cannot poſitively aſſert, that the old queens yield eggs ſo plentifully at all times,

Theſe eggs are inſtantly taken from her body by her attendants (of whom there always are, in the royal chamber and the galleries adjacent, a ſufficient number in waiting) and carried to the nurſeries, which in a great neſt may ſome of them be four or five feet diſtant in a ſtraight line, and conſequently much farther by their winding galleries. Here, after they are hatched, the young are attended and provided with every thing neceſſary until they are able to ſhift for themſelves, and take their ſhare of the labours of the community. The foregoing, I flatter myſelf, is an accurate deſcription and account of the *Termes bellicoſus* or ſpecies that builds the large neſts in its different ſtates.

Thoſe which build either the roofed turrets or the neſts in the trees, ſeem in moſt inſtances to have a ſtrong reſemblance to them, both in their form and oeconomy, going through the ſame changes from the egg to the winged ſtate. The *queens* alſo increaſe to a great ſize when compared with the labourers; but very ſhort of thoſe *queens* before deſcribed. The largeſt are from about an inch to an inch and a half long; and not much thicker than a common quill. There is the ſame kind of periſtaltic motion in the abdomen, but in a much ſmaller de-

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times, but the protruding them being the conſequence of the periſtaltic motion, it would ſeem involuntary on their parts, and the number, or nearly ſo, always indiſpenſable: the aſtoniſhing multitudes of inhabitants found in their neſts alſo countenance this opinion ſtrongly.

(29) Since the reading of this paper, Mr. JOHN HUNTER, ſo celebrated for his great ſkill and experience in comparative anatomy, has diſſected two young queens. He finds the abdomen contains two ovaria, in each of which are many hundred ova-ducts, and in each of theſe ova-ducts a vaſt many eggs; ſo that there ſeems no doubt of the fact, as the matrix of a *full-grown queen* muſt be calculated for the production and yielding of a prodigious number of eggs. He has alſo diſſected the kings; The reſult of theſe diſſections, with ſome further particulars, will be related in another paper.

gree; and, as the animal is incapable of moving from her place, the eggs no doubt are carried to the different cells by the labourers, and reared with a care ſimilar to that which is practiſed in the larger neſts.

It is remarkable of all theſe different ſpecies, that the working and the fighting inſects never expoſe themſelves to the open air; but either travel under ground, or within ſuch trees and ſubſtances as they deſtroy, except, indeed, when they cannot proceed by their latent paſſages, and find it convenient or neceſſary to ſearch for plunder above ground. In that caſe they make pipes of that material with which they build their neſts. The larger ſort uſe the red clay; the turret builders uſe the black clay; and thoſe which build in the trees employ the ſame ligneous ſubſtances of which their neſts are compoſed (30) (31) (32).

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(30) "Small birds, fowls, Lizards, and other reptiles, ſearch for them as the moſt delicious morſels; therefore they never go abroad but under their covered ways." DU TERTRE, quarto, vol. II. p. 345.

(31) "The earth hereabouts was all filled with a ſpecies of a white Ant, called Vag Vague, different from that which I have elſewhere deſcribed. This, inſtead of raiſing pyramids, continues buried under ground, and never makes itſelf known but by ſmall cylindrical galleries of the thickneſs of a gooſe quill, which it erects againſt the ſeveral bodies it deſigns to attack. Theſe galleries are formed of earth with infinite delicacy of workmanſhip. The Vag Vagues make uſe of them as of covert-ways, to work without being ſeen; and whatever they faſten themſelves to, whether it be leather, cloth, linen, books, or wood, it is ſurely gnawed and conſumed. I ſhould have thought myſelf pretty well off, had they only attacked the reeds of my hut; but they pierced through a trunk which ſtood on treſtles a foot above the ground, and gnawed moſt of my books." ADANSON's Voyage to Guinea, 179—337.

N. B. Mr. ADANSON is certainly miſtaken when he ſays, "They never make themſelves known but by their covered ways, and is the only one whom I have

" met

With theſe materials they completely line moſt of the roads leading from their neſts into the various parts of the country, and travel out and home with the utmoſt ſecurity in all kinds of weather. If they meet a rock or any other obſtruction, they will make their way upon the ſurface; and for that purpoſe erect a covered way or arch, ſtill of the ſame materials, continuing it with many windings and ramifications through large groves; having, where it is poſſible, ſubterranean pipes running parallel with them, into which they ſink and ſave themſelves, if their galleries above ground are deſtroyed by any violence, or the tread of men or animals alarms them. When one chances by accident to enter any ſolitary grove, where the ground is pretty well covered with their arched galleries, they give the alarm by loud hiſſings, which we hear diſtinctly at every ſtep we make; ſoon after which we may examine their galleries in vain for the inſects, but find little holes, juſt large enough for them, by which they have made their eſcape into their ſubterraneous roads. Theſe galleries are large enough for them to paſs and repaſs ſo as to prevent any ſtoppages (though there are always numerous paſſengers) and ſhelter them equally from light and

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" met with who has been attacked while living by the *white Ants.*" I have ſome doubt, that, although the approaches of the *Termites* were carried up to his bed, the bites he received were from *real Ants*, of which there are ſome ſcarce viſible which are very numerous and produce great pain; whereas the bite of the *Termes* lets out much blood, and ſhews not the leaſt ſymptom of venom. See DU TERTRE's Antilles, vol. II. p. 344. and Deſcript. de l'Afrique, par LABAT, tom. III. p. 298.

(32) See SLOANE, LIGON, LINNÆUS (Termes Fatalis), FORSKAL (Termes Arda), and the various voyages to Africa and both Indies.

air, as well as from their enemies, of which the ants, being the moſt numerous, are the moſt formidable.

The *Termites*, except their heads, are exceeding ſoft, and covered with a very thin and delicate ſkin; being blind, they are no match on open ground for the ants, who can ſee, and are all of them covered with a ſtrong horny ſhell not eaſily pierced, and are of diſpoſitions bold, active, and rapacious. Whenever the Termites are diſlodged from their covered ways, the various ſpecies of the former, who probably are as numerous above ground as the latter are in their ſubterraneous paſſages, inſtantly ſeize and drag them away to their neſts, to feed the young brood (33) (34) (35). The Termites are therefore exceeding

ſolicitous

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(33) Sir HANS SLOANE was certainly miſtaken in his account of the Wood Ants; it is utterly improbable that they ſhould go into the neſts of the red Ants and kill them. It is moſt probable, the error has ariſen from Sir HANS's having confounded the two genera of inſects the *Formica* and *Termes* together, which made him never ſpeak of them with preciſion. The reverſe of his account is moſt likely, which is, that the *Formicæ* will follow their plunder into the neſts of the *Termites* and deſtroy them; for the latter always keep within their neſts or covered ways, avoiding all communications with other inſects and animals, and never meddling with them but when dead; whereas the *Formicæ* ramble about every where, and enter every cranny and hole that is large enough, and attack not only inſects and reptiles but even large animals. See SLOANE's Voyage to Jamaica, vol. II. p. 221, 222. tab. 238. *Hiſt. de l'Academie Royale des Sciences*, 1701, p. 16. *Fourmis de Viſite.*

(34) LIGON mentions another *ſort* of *Ants*, and deſcribes the galleries of the *Termites*. LIGON's Barbadoes, p. 64, 65.

(35) MERIAN ſays, the *Ants* make neſts above eight feet high, by which I apprehend ſhe means the neſts of the *Termites*; but in ſpeaking of the manners of the inſects ſhe certainly means ſome ſpecies of the *Formica*. Thoſe which are deſcribed as ſtripping the trees are a ſpecies called, in Tobago, *Para-ſol-Ants*, becauſe they cut out of the leaves of certain trees and plants pieces almoſt circular,

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ſolicitous about the preſerving their covered ways in good repair; and if you demoliſh one of them, for a few inches in length, it is wonderful how ſoon they rebuild it. At firſt in their hurry they get into the open part an inch or two, but ſtop ſo ſuddenly that it is very apparent they are ſurprized: for though ſome run ſtraight on, and get under the arch as ſpeedily as poſſible in the further part, moſt of them run as faſt back, and very few will venture through that part of the track which is left uncovered. In a few minutes you will perceive them rebuilding the arch, and by the next morning they will have reſtored their gallery for three or four yards in length, if ſo much has been ruined; and upon opening it again will be found as numerous as ever, under it, paſſing both ways. If you continue to deſtroy it ſeveral times, they will at length ſeem to give up the point, and build another in a different direction; but, if the old one led to ſome favourite plunder, in a few days will rebuild it again; and, unleſs you deſtroy their neſt, never totally abandon their gallery.

The *Termites arborum*, thoſe which build in trees, frequently eſtabliſh their neſts within the roofs and other parts of houſes, to which they do conſiderable damage, if not timely extirpated.

The large ſpecies are, however, not only much more deſtructive, but more difficult to be guarded againſt, ſince they make their approaches chiefly under ground, deſcending below the foundations of houſes and ſtores at ſeveral feet from the ſurface, and riſing again either in the floors, or entering at the

and are to be ſeen all the year round travelling from the plants along their road to the neſt, with each one of theſe circular pieces of leaves in their jaws, which, from their ſhape and colour, give a very good idea of people walking with paraſols (umbrellas). MERIAN, *Inſectes de Surinam*, p. 18.

bottoms of the poſts, of which the ſides of the buildings are compoſed, bore quite through them, following the courſe of the fibres to the top, or making lateral perforations and cavities here and there as they proceed.

While ſome are employed in gutting the poſts, others aſcend from them, entering a rafter or ſome other part of the roof. If they once find the thatch, which ſeems to be a favourite food, they ſoon bring up wet clay, and build their pipes or galleries through the roof in various directions, as long as it will ſupport them; ſometimes eating the palm-tree leaves and branches of which it is compoſed, and, perhaps (for variety ſeems very pleaſing to them) the rattan or other running plant which is uſed as a cord to tye the various parts of the roof together, and that to the poſts which ſupport it: thus, with the aſſiſtance of the rats, who during the rainy ſeaſon are apt to ſhelter themſelves there, and to burrow through it, they very ſoon ruin the houſe by weakening the faſtenings and expoſing it to the wet. In the mean time the poſts will be perforated in every direction as full of holes as that timber in the bottoms of ſhips which has been bored by the worms; the fibrous and knotty parts, which are the hardeſt, being left to the laſt (36).

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(36) The ſea worms, ſo pernicious to our ſhipping, appear to have the ſame office allotted them in the waters which the Termites have on the land. They will appear, on a very little conſideration, to be moſt important beings in the great chain of creation, and pleaſing demonſtrations of that infinitely wiſe and gracious Power which formed, and ſtill preſerves, the whole in ſuch wonderful order and beauty: for if it was not for the rapacity of theſe and ſuch animals, tropical rivers, and indeed the ocean itſelf, would be choked with the bodies of trees which are annually carried down by the rapid torrents, as many of them would laſt for ages, and probably be productive of evils, of which, happily, we

They ſometimes, in carrying on this buſineſs, find, I will not pretend to ſay how, that the poſt has ſome weight to ſupport, and then if it is a convenient track to the roof, or is itſelf a kind of wood agreeable to them, they bring their mortar, and fill all or moſt of the cavities, leaving the neceſſary roads through it, and as faſt as they take away the wood replace the vacancy with that material; which being worked together by them cloſer and more compactly than human ſtrength or art could ram it, when the houſe is pulled to pieces, in order to examine if any of the poſts are fit to be uſed again, thoſe of the ſofter kinds are often found reduced almoſt to a ſhell, and all or a greater part transformed from wood to clay as ſolid and as hard as many kinds of free-ſtone uſed for building in England. It is much the ſame when the *Termites bellicoſi* get into a cheſt or trunk containing cloaths and other things; if the weight

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we cannot in the preſent harmonious ſtate of things form any idea[*]; whereas now being conſumed by theſe animals, they are more eaſily broken in pieces by the waves; and the fragments which are not devoured become ſpecifically lighter, and are conſequently more readily and more effectually thrown on ſhore, where the ſun, wind, inſects, and various other inſtruments, ſpeedily promote their entire diſſolution, and reſtore the conſtituent particles to that

——— "Mighty hand,
"Which, ever buſy, wheels the ſilent ſpheres;
"Works in the ſecret deep; ſhoots, ſteaming, thence
"The fair profuſion that o'erſpreads the ſpring:
"Flings from the ſun direct the flaming day;
"Feeds every creature; hurls the tempeſt forth;
"And, as on earth this grateful change revolves,
"With tranſport touches all the ſprings of life." THOMSON.

[*] That wood will endure in water an amazing number of ages, is apparent from the *oak ſtakes* which were driven into the bed of the river Thames on the invaſion of this iſland by *Julius Cæſar*, one of which is to be ſeen in Sir ASHTON LEVER's Muſeum, and likewiſe from thoſe bodies of trees which are daily found in the bogs and moraſſes of Great Britain and Ireland, which after a duration, the former of eighteen hundred, the latter of upwards of two thouſand years, are found in a perfect ſtate of preſervation.

above is great, or they are afraid of Ants or other enemies, and have time, they carry their pipes through, and replace a great part with clay, running their galleries in various directions. The tree Termites, indeed, when they get within a box, often make a neſt there, and being once in poſſeſſion deſtroy it at their leiſure. They did ſo to the pyramidal box which contained my compound microſcope. It was of mahogany, and I had left it in the ſtore of Governor CAMPBELL of Tobago, for a few months, while I made the tour of the Leeward Iſlands. On my return I found theſe inſects had done much miſchief in the ſtore, and, among other things, had taken poſſeſſion of the microſcope, and eaten every thing about it except the glaſs or metal, and the board on which the pedeſtal is fixed, with the drawers under it, and the things incloſed. The cells were built all round the pedeſtal and the tube, and attached to it on every ſide. All the glaſſes which were covered with the wooden ſubſtance of their neſts retained a cloud of a gummy nature upon them that was not eaſily got off, and the lacquer or burniſh with which the braſs work was covered was totally ſpoiled. Another party had taken a liking to the ſtaves of a Madeira caſk, and had let out almoſt a pipe of fine old wine. If the large ſpecies of Africa (the *Termites bellicoſi*) had been ſo long in the uninterrupted poſſeſſion of ſuch a ſtore, they would not have left twenty pounds weight of wood remaining of the whole building, and all that it contained (37).

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(37) Captain PHILLIP of the navy, who was ſome time at the Brazils in the ſervice of Portugal, gives me the following relation. "An engineer returned "from ſurveying the country, left his trunk on a table: the next morning, not "only all his cloaths were deſtroyed by *white Ants* or *Cutters*, but his papers alſo; "and the latter in ſuch a manner, that there was not a bit left of an inch ſquare.

Theſe inſects are not leſs expeditious in deſtroying the ſhelves, wainſcotting, and other fixtures of an houſe, than the houſe itſelf. They are for ever piercing and boring in all directions, and ſometimes go out of the broadſide of one poſt into that of another joining to it; but they prefer and always deſtroy the ſofter ſubſtances the firſt, and are particularly fond of pine and fir-boards, which they excavate and carry away with wonderful diſpatch and aſtoniſhing cunning: for, except a ſhelf has ſomething ſtanding upon it, as a book, or any thing elſe which may tempt them, they will not perforate the ſurface, but artfully preſerve it quite whole, and eat away all the inſide, except a few fibres which barely keep the two ſides connected together, ſo that a piece of an inch-board which appears ſolid to the eye will not weigh much more than two ſheets of paſteboard of equal dimenſions, after theſe animals have been a little while in poſſeſſion of it (38) (39) (40) (41). In ſhort, the Termites are

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" The black lead pencils were likewiſe ſo completely deſtroyed, that the ſmalleſt " piece, even of the *black lead* could not be found. The cloaths were not " entirely cut to pieces and carried away, but appeared as if moth-eaten, there " being ſcarce a piece as large as a ſhilling that was free from ſmall holes; and " it was further remarkable, that ſome *ſilver coin*, which was in the trunk, had a " number of black ſpecks on it, cauſed by ſomething ſo corroſive that they could " not eaſily be rubbed off even with ſand." Queen's-ſquare, Wedneſday, Jan. 17, 1781.

(38) " The white Ants are tranſparent as glaſs, and bite ſo forcibly, that in " the ſpace of one night alone they can eat their way through a thick wooden " cheſt of goods, and make it as full of holes, as if it had been ſhot through " with hail-ſhot." BOSMAN's Guinea, p. 276, 7. 493.

(39) MOORE's Travels, p. 221.

(40) Voyage de LABAT aux Iſles, tom. II. p. 331.

(41) " The wood Ants are the moſt pernicious of all others, being ſo very " deſtructive to timber of moſt ſorts, that, if not prevented, they will in a few

" years

ſo inſidious in their attacks, that we cannot be too much on our guard againſt them: they will ſometimes begin and raiſe their works, eſpecially in new houſes, through the floor (42). If you deſtroy the work ſo begun, and make a fire upon the ſpot, the next night they will attempt to riſe through another part; and, if they happen to emerge under a cheſt or trunk early in the night, will pierce the bottom, and deſtroy or ſpoil every thing in it before the morning (43). On theſe accounts we are careful to ſet all our cheſts and boxes upon ſtones or bricks, ſo as to leave the bottoms of ſuch furniture ſome inches above the ground; which not only prevents theſe inſects finding them out ſo readily, but preſerves the bottoms from a corroſive damp which would ſtrike from the earth through, and rot every thing therein: a vaſt deal of vermin alſo would harbour under, ſuch as Cock-roaches, Centipedes, Millepedes, Scorpions, Ants, and various other noiſome inſects.

When the Termites attack trees and branches in the open air, they ſometimes vary their manner of doing it. If a ſtake in a hedge has not taken root and vegetated, it becomes their buſineſs to deſtroy it. If it has a good ſound bark round it, they

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" years time deſtroy the whole roof of an houſe, eſpecially if it be of ſoft tim-
" ber.—— They have likewiſe cauſed great loſſes to ſhop-keepers, by boring
" through whole bales of linnen as well as woolen cloths. HUGHES's Barbadoes, p. 93.

(42) The floors are generally made of the ſtone or clay taken from the hills raiſed by theſe inſects, which, being moiſtened with water, and mixed by treading, is beaten level, ſmooth, and compact, with their feet and a kind of hand-bat or beetle.

(43) " One night, in a few hours, they pierced one foot of the table, and
" (having in that manner aſcended) carried their arch acroſs it, and then
" down through the middle of the other foot into the floor, as good luck would
" have it, without doing any damage to the papers left there." KEMPFER Hiſt. Japan, vol. II. p. 127.

will enter at the bottom, and eat all but the bark, which will remain, and exhibit the appearance of a ſolid ſtick (which ſome vagrant colony of Ants or other inſects often ſhelter in till the winds diſperſe it); but if they cannot truſt the bark, they cover the whole ſtick with their mortar, and it then looks as if it had been dipped into thick mud that had been dried on. Under this covering they work, leaving no more of the ſtick and bark than is barely ſufficient to ſupport it, and frequently not the ſmalleſt particle, ſo that upon a very ſmall tap with your walking ſtick, the whole ſtake, though apparently as thick as your arm, and five or ſix feet long, loſes its form, and diſappearing like a ſhadow falls in ſmall fragments at your feet. They generally enter the body of a large tree which has fallen through age or been thrown down by violence, on the ſide next the ground, and eat away at their leiſure within the bark, without giving themſelves the trouble either to cover it on the outſide, or to replace the wood which they have removed from within, being ſomehow ſenſible that there is no neceſſity for it. Theſe excavated trees have deceived me two or three times in running: for, attempting to ſtep two or three feet high, I might as well have attempted to ſtep upon a cloud, and have come down with ſuch unexpected violence, that, beſides ſhaking my teeth and bones almoſt to diſlocation, I have been precipitated, head foremoſt, among the neighbouring trees andbuſhes. Sometimes, though ſeldom, the animals are known to attack living trees; but not, I apprehend, before ſymptoms of mortification have appeared at the roots, ſince it is evident, as is before obſerved, that theſe inſects are intended in the order of nature to haſten the diſſolution of ſuch trees and vegetables as have arrived at their greateſt maturity and perfection,

fection, and which would, by a tedious decay, ſerve only to encumber the face of the earth. This purpoſe they anſwer ſo effectually, that nothing periſhable eſcapes them, and it is almoſt impoſſible to leave any thing penetrable upon the ground a long time in ſafety; for the odds are, that, put it where you will abroad, they will find it out before the following morning, and its deſtruction follows very ſoon of courſe. In conſequence of this diſpoſition, the woods never remain long encumbered with the fallen trunks of trees or their branches; and thus it is, as I have before obſerved, the total deſtruction of deſerted towns is ſo effectually completed, that in two or three years a thick wood fills the ſpace; and, unleſs *iron-wood* poſts have been made uſe of, not the leaſt veſtige of an houſe is to be diſcovered.

The firſt object of admiration which ſtrikes one upon opening their hills is the behaviour of the ſoldiers. If you make a breach in a ſlight part of the building, and do it quickly with a ſtrong hoe or pick-axe, in the ſpace of a few ſeconds a ſoldier will run out, and walk about the breach, as if to ſee whether the enemy is gone, or to examine what is the cauſe of the attack. He will ſometimes go in again, as if to give the alarm: but moſt frequently, in a ſhort time, is followed by two or three others, who run as faſt as they can, ſtraggling after one another, and are ſoon followed by a large body who ruſh out as faſt as the breach will permit them, and ſo they proceed, the number increaſing, as long as any one continues battering their building (44). It is not eaſy to deſcribe the rage and fury they ſhew. In their hurry they frequently miſs their hold, and tumble down the ſides of the hill, but recover themſelves as quickly as poſſible; and, being blind, bite every thing they run againſt, and thus make a crackling noiſe, while ſome of them beat repeatedly with their forceps upon the building, and make a ſmall vibrating noiſe, ſomething ſhriller and quicker than the ticking of a watch: I could diſtinguiſh this noiſe at three or four feet diſtance, and it continued for a minute at a time, with ſhort intervals. While the attack proceeds they are in the moſt violent buſtle and agitation. If they get hold of any one, they will in an inſtant let out blood enough to weigh againſt their whole body; and if it is the leg they wound, you will ſee the ſtain upon the ſtocking extend an inch in width. They make their hooked jaws meet at the firſt ſtroke, and never quit their hold, but ſuffer themſelves to be pulled away leg by leg, and piece after piece, without the leaſt attempt to eſcape. On the other hand, keep out of their way, and give them no interruption, and they will in leſs than half an hour retire into the neſt, as if they ſuppoſed the wonderful monſter that damaged their caſtle to be gone beyond their reach. Before they are all got in you will ſee the labourers in motion, and haſtening in various directions toward the breach: every one with a burthen of mortar in his mouth ready tempered. This they ſtick upon the breach as faſt as they come up, and do it with ſo much diſpatch and facility, that although there are thouſands, and I may ſay millions, of them, they never ſtop or embarraſs

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(44) "They throw up little hills of ſeven or eight feet high, ſo very full of "holes that they rather ſeem like honey-combs than burrows. Theſe Ant hills "are of a very ſmall circumference in proportion to their height, being ſharp at top, "ſo that to judge by the looks of them one would think the wind could blow them "down.

" down. I one day attempted to knock off the top of one of them with my cane, " but the ſtroke had no other effect than to bring ſome thouſands of the animals " out of doors, to ſee what was the matter: upon which I took to my heels and " ran away as faſt as I could." SMITH's Voyage to Guinea.

embarraſs one another; and you are moſt agreeably deceived when, after an apparent ſcene of hurry and confuſion, a regular wall ariſes, gradually filling up the chaſm. While they are thus employed, almoſt all the ſoldiers are retired quite out of ſight, except here and there one, who ſaunters about among ſix hundred or a thouſand of the labourers, but never touches the mortar either to lift or carry it; one, in particular, places himſelf cloſe to the wall they are building. This ſoldier will turn himſelf leiſurely on all ſides, and every now and then, at intervals of a minute or two, lift up his head, and with his forceps beat upon the building, and make the vibrating noiſe before mentioned; on which immediately a loud hiſs, which appears to come from all the labourers, iſſues from within ſide the dome and all the ſubterraneous caverns and paſſages: that it does come from the labourers is very evident, for you will ſee them all haſten at every ſuch ſignal, redouble their pace, and work as faſt again.

As the moſt intereſting experiments become dull by repetition or continuance, ſo the uniformity with which this buſineſs is carried on, though ſo very wonderful, at laſt ſatiates the mind. A renewal of the attack, however, inſtantly changes the ſcene, and gratifies our curioſity ſtill more. At every ſtroke we hear a loud hiſs; and on the firſt the labourers run into the many pipes and galleries with which the building is perforated, which they do ſo quickly that they ſeem to vaniſh, for in a few ſeconds all are gone, and the ſoldiers ruſh out as numerous and as vindictive as before (45). On finding no enemy they return again leiſurely into the hill, and very ſoon after the labourers appear loaded as at firſt, as active and as ſedulous, with ſoldiers here and there among them, who act juſt in the ſame manner, one or other of them giving the ſignal to haſten the buſineſs. Thus the pleaſure of ſeeing them come out to fight or to work alternately may be obtained as often as curioſity excites or time permits: and it will certainly be found, that the one order never attempts to fight, or the other to work, let the emergency be ever ſo great.

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(45) By the ſoldiers being ſo ready to run out upon the repetition of the attack, it appears, that they but juſt withdraw out of ſight, to leave room for the labourers to proceed without interruption in repairing the breach, and in this inſtance

We meet vaſt obſtacles in examining the interior parts of theſe tumuli. In the firſt place, the works, for inſtance, the apartments which ſurround the royal chamber and the nurſeries, and indeed the whole internal fabric, are moiſt, and conſequently the clay is very brittle: they have alſo ſo cloſe a connexion, that they can only be ſeen as it were by piece-meal; for having a kind of geometrical dependance or abutment againſt each other, the breaking of one arch pulls down two or three. To theſe obſtacles muſt be added the obſtinacy of the ſoldiers, who fight to the very laſt, diſputing every inch of ground ſo well as often to drive away the negroes who are without ſhoes, and make white people bleed plentifully through their ſtockings. Neither can we let a building ſtand ſo as to get a view of the interior parts without interruption, for while the ſoldiers are defending the

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inſtance they ſhew more good ſenſe than the bulk of mankind, for, in caſe of a conflagration in a city, the number of people who aſſemble to ſtare is much greater than of thoſe who come to aſſiſt, and the former always interrupt and hinder the latter in their efforts. The ſudden retreat of the labourers, in caſe of an alarm, is alſo a wonderful inſtance of good order and diſcipline, ſeldom ſeen in populous cities, where we frequently find helpleſs people, women, and children, without any ill intention, intermixing in violent tumults and dangerous riots.

out-works, the labourers keep barricadoing all the way againſt us, ſtopping up the different galleries and paſſages which lead to the various apartments, particularly the royal chamber, all the entrances to which they fill up ſo artfully as not to let it be diſtinguiſhable while it remains moiſt; and externally it has no other appearance than that of a ſhapeleſs lump of clay (46). It is, however, eaſily found from its ſituation with reſpect to the other parts of the building, and by the crouds of labourers and ſoldiers which ſurround it, who ſhew their loyalty and fidelity by dying under its walls. The royal chamber in a large neſt is capacious enough to hold many hundreds of the attendants, beſides the royal pair, and you always find it as full of them as it can hold. Theſe faithful ſubjects never abandon their charge even in the laſt diſtreſs; for whenever I took out the royal chamber, and, as I often did, preſerved it for ſome time in a large glaſs bowl, all the attendants continued running in one direction round the king and queen with the utmoſt ſolicitude, ſome of them ſtopping on every circuit at the head of the latter, as if to give her ſomething. When they came to the extremity of the abdomen, they took the eggs from her, and carried them away, and piled them carefully together in ſome part of the chamber, or in the bowl under, or behind any pieces of broken clay which lay moſt convenient for the purpoſe.

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(46) In tab. VIII. fig. 2. and 4. the entrances of the royal chamber, now exhibited, are repreſented open. They were all ſhut by the labourers before I had got to it, and were opened ſince I arrived in England. Two or three of them, however, are not quite open in the chamber itſelf, and that next the breach at A, and marked with a croſs ⊕, is ſtill left ſhut, as a ſpecimen of the manner in which they do it. I have alſo more royal chambers and various ſpecimens of the interior buildings, with ſeveral galleries and paſſages, ſhut up while we were attacking the neſt.

Some of theſe little unhappy creatures would ramble from the chamber, as if to explore the cauſe of ſuch a horrid ruin and cataſtrophe to their immenſe building, as it muſt appear to them; and, after fruitleſs endeavours to get over the ſide of the bowl, return and mix with the croud that continue running round their common parents to the laſt (tab. VIII. fig. 4. B.). Others, placing themſelves along her ſide, get hold of the queen's vaſt matrix with their jaws, and pull with all their ſtrength ſo as viſibly to lift up the part which they fix at; but, as I never ſaw any effect from theſe attempts, I never could determine whether this pulling was with an intention to remove her body, or to ſtimulate her to move herſelf, or for any other purpoſe; but, after many ineffectual tugs, they would deſiſt and join in the croud running round, or aſſiſt ſome of thoſe who are cutting off clay from the external parts of the chamber or ſome of the fragments and moiſtening it with the juices of their bodies, to begin to work a thin arched ſhell over the body of the queen, as if to exclude the air, or to hide her from the obſervation of ſome enemy. Theſe, if not interrupted, before the next morning, completely cover her, leaving room enough within for great numbers to run about her.

I do not mention the king in this caſe, becauſe he is very ſmall in proportion to the queen, not being bigger than thirty of the labourers, ſo that he generally conceals himſelf under one ſide of the abdomen, except when he goes up to the queen's head, which he does now and then, but not ſo frequently as the reſt.

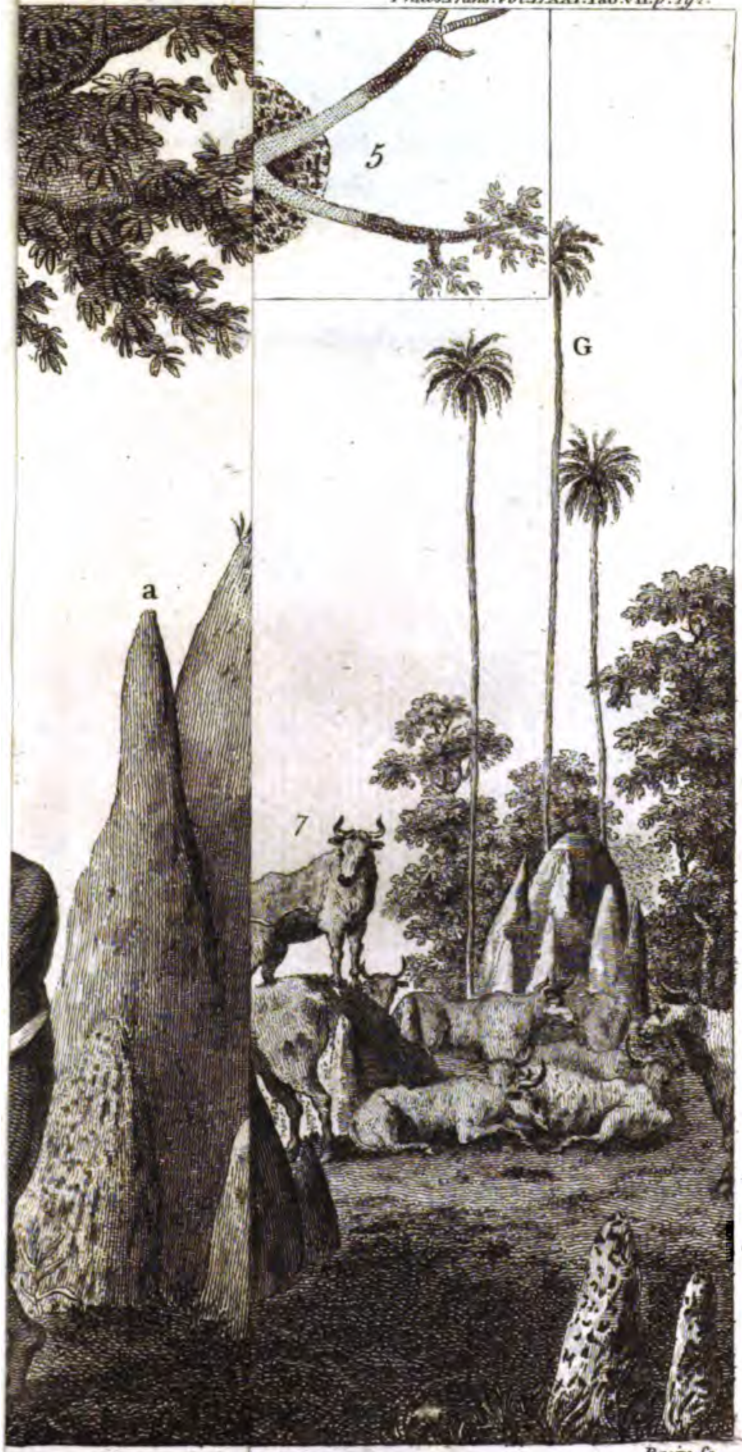
If in your attack on the hill you ſtop ſhort of the royal chamber, and cut down about half of the building, and leave open ſome thouſands of galleries and chambers, they will all be ſhut up with thin ſheets of clay before the next morning. If even the whole is pulled down, and the different buildings are thrown in a confuſed heap of ruins, provided the king and queen are not deſtroyed or taken away, every interſtice between the ruins, at which either cold or wet can poſſibly enter, will be ſo covered as to exclude both, and, if the animals are left undiſturbed, in about a year they will raiſe the building to near its priſtine ſize and grandeur.

The marching Termites are not leſs curious in their order, as far as I have had an opportunity of obſerving them, than thoſe deſcribed before. This ſpecies ſeems much ſcarcer and larger than the *Termes bellicoſus*. I could get no information relative to them from the black people, from which I conjecture they are little known to them: my ſeeing them was very accidental. One day, having made an excurſion with my gun up the river Camerankoes, on my return through the thick foreſt, whilſt I was ſauntering very ſilently in hopes of finding ſome ſport, on a ſudden I heard a loud hiſs, which, on account of the many ſerpents in thoſe countries, is a moſt alarming ſound. The next ſtep cauſed a repetition of the noiſe, which I ſoon recognized, and was rather ſurpriſed ſeeing no covered ways or hills. The noiſe, however, led me a few paces from the path, where, to my great aſtoniſhment and pleaſure, I ſaw an army of Termites coming out of a hole in the ground, which could not be above four or five inches wide. They came out in vaſt numbers, moving forward as faſt ſeemingly as it was poſſible for them to march. In leſs than a yard from this place they divided into two ſtreams or columns, compoſed chiefly of the firſt order, which I call labourers, twelve or fifteen a-breaſt, and crouded as cloſe after one another as ſheep in a drove, going ſtraight forward without deviating to the right or left. Among theſe, here and there, one of the ſoldiers was to be ſeen, trudging along with them, in the ſame manner, neither ſtopping or turning; and as he carried his enormous large head with apparent difficulty, he put me in mind of a very large ox amidſt a flock of ſheep. While theſe were buſtling along, a great many ſoldiers were to be ſeen ſpread about on both ſides of the two lines of march, ſome a foot or two diſtant, ſtanding ſtill or ſauntering about as if upon the look out leaſt ſome enemy ſhould ſuddenly come upon the labourers. But the moſt extraordinary part of this march was the conduct of ſome others of the ſoldiers, who having mounted the plants which grow thinly here and there in the thick ſhade, had placed themſelves upon the points of the leaves, which were elevated ten or fifteen inches above the ground, and hung over the army marching below. Every now and then one or other of them beat with his forceps upon the leaf, and made the ſame ſort of ticking noiſe which I had ſo frequently obſerved to be made by the ſoldier who acts the part of a ſurveyor or ſuper-intendant when the labourers are at work repairing a breach made in one of the common hills of the *Termites bellicoſi.* This ſignal among the marching white Ants produced a ſimilar effect; for, whenever it was made, the whole army returned a hiſs, and obeyed the ſignal by increaſing their pace with the utmoſt hurry. The ſoldiers who had mounted aloft, and gave theſe ſignals, ſat quite ſtill during the intervals (except making now and then a ſlight turn of the head) and ſeemed as ſolicitous to keep their poſts as regular centinels. The two columns of the army joined into one about twelve or fifteen paces from their ſeparation, having in no part been above three yards aſunder, and then deſcended into the earth by two or three holes. They continued marching by me for above an hour that I ſtood admiring them, and ſeemed neither to increaſe or diminiſh their numbers, the ſoldiers only excepted, who quitted the line of march, and placed themſelves at different diſtances on each ſide of the two columns; for they appeared much more numerous before I quitted the ſpot. Not expecting to ſee any change in their march, and being pinched for time, the tide being nearly up, and our departure fixed at high water, I quitted the ſcene with ſome regret, as the obſervation of a day or two might have afforded me the opportunity of exploring the reaſon and neceſſity of their marching with ſuch expedition, as well as of diſcovering their chief ſettlement, which is probably built in the ſame manner as the large hills before deſcribed. If ſo, it may be larger and more curious, as theſe inſects were at leaſt one-third larger than the other ſpecies, and conſequently their buildings muſt be more wonderful if poſſible: thus much is certain, there muſt be ſome fixed place for their king and queen, and the young ones. Of theſe ſpecies I have not ſeen the perfect inſect.

The œconomy of nature is wonderfully diſplayed in a comparative obſervation on the different ſpecies who are calculated to live under ground until they have wings, and this ſpecies which marches in great bodies in open day. The former, in the two firſt ſtates, that is, of labourers and ſoldiers, have no eyes that I could ever diſcover; but when they arrive at the winged or perfect ſtate in which they are to appear abroad, though only for a few hours, and that chiefly in the night, they are furniſhed with two conſpicuous and fine eyes: ſo the *Termes viarum*, or marching Bugga Bugs, being intended to walk in the open air and light, are even in the firſt ſtate furniſhed with eyes proportionably as fine as thoſe which are given to the winged or perfect inſects of the other ſpecies.

I am

Philos. Trans. Vol. LXXI. Tab. VII. p. 192.



Basire Sc.

with which they were ſtopped up, has been in part or wholly picked out with a ſmall inſtrument, p. 187.

Fig. 5. A nurſery, p. 153.

Fig. 6. A little nurſery, with the eggs, the young ones, the muſhrooms, mouldineſs, &c. as juſt taken from the hill. p. 153.

Fig. 7. The muſhrooms magnified by a ſtrong lens, p. 154.

Tab. IX. fig. 1. and 2. The turret neſts, with roofs of the Termes mordax and a Termes atrox as finiſhed, p. 159.

Fig. 3. A turret, with the roof begun.

Fig. 4. A turret, raiſed only about half its height.

Fig. 5. A turret, building upon one which had been thrown down, p. 160.

Fig. 6. 6. A turret, broken in two.

Tab. X. fig. 1. A Termes bellicoſus, p. 141. numb. 1. and p. 165.

Fig. 2. A KING. N. B. A king never alters his form after he loſes his wings, neither does he apparently increaſe in bulk.

Fig. 3. A QUEEN, p. 170.

Fig. 4. The head of a perfect inſect magnified.

Fig. 5. A face, with ſtemmata magnified, p. 141. numb. 1.

Fig. 6. A labourer, p. 163.

Fig. 7. A labourer magnified.

Fig. 8. A ſoldier, p. 164.

Fig. 9. A ſoldier's forceps and part of his head magnified, p. 164.

Fig. 10. The Termes mordax, p. 141. numb. 2. and p. 161.

Fig. 11. The face with the ſtemmata magnified, p. 141. numb. 2.

Fig. 12. A labourer.

Fig. 13. A ſoldier.

Fig. 14. The Termes atrox, p. 141. numb. 3. and p. 160.

Fig. 15. The face and ſtemmata magnified, p. 141. numb. 3.

Fig. 16. A labourer.

Fig. 17. A ſoldier.

Fig. 18. Idem.

Fig. 19. The Termes deſtructor, p. 141. numb. 4.

Fig. 20. The face and ſtemmata magnified, p. 141. numb. 4.

Fig. 21. The Termes arborum, p. 141. numb. 5. and p. 162.

Fig. 22. The face and ſtemmata magnified, p. 141. numb. 5.

Fig. 23. A labourer.

Fig. 24. A ſoldier.

Fig. 25. A QUEEN, p. 172.

N. B. In the figures 5. 11. 15. 20. and 21. the two white ſpots between the edges are the ſtemmata.

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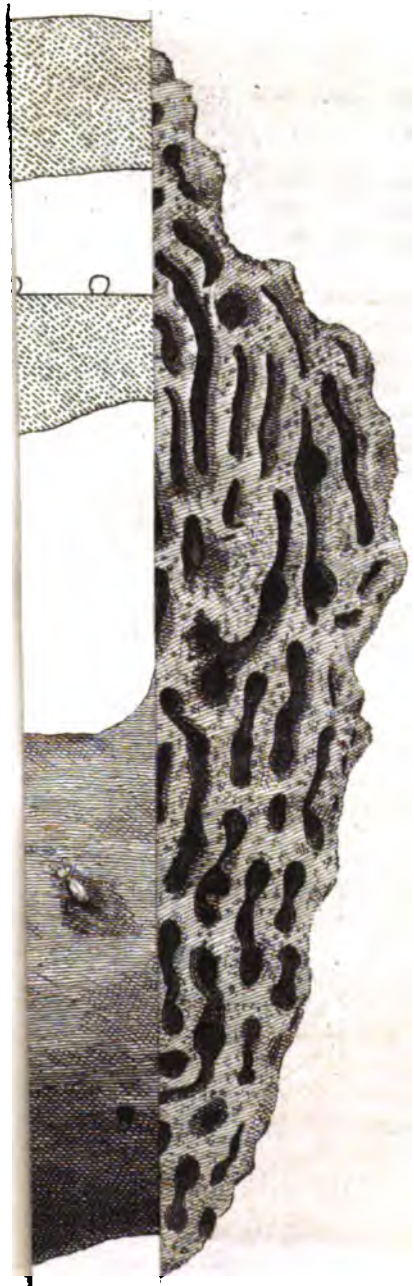
Philos. Trans. Vol. LXXI. Tab. VIII. p. 192.



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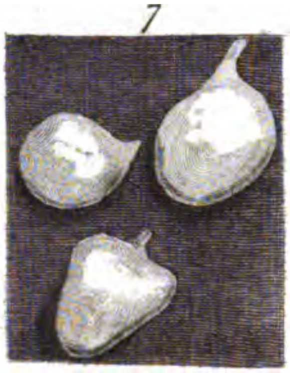

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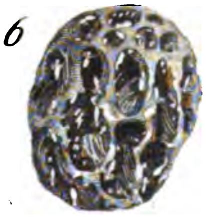

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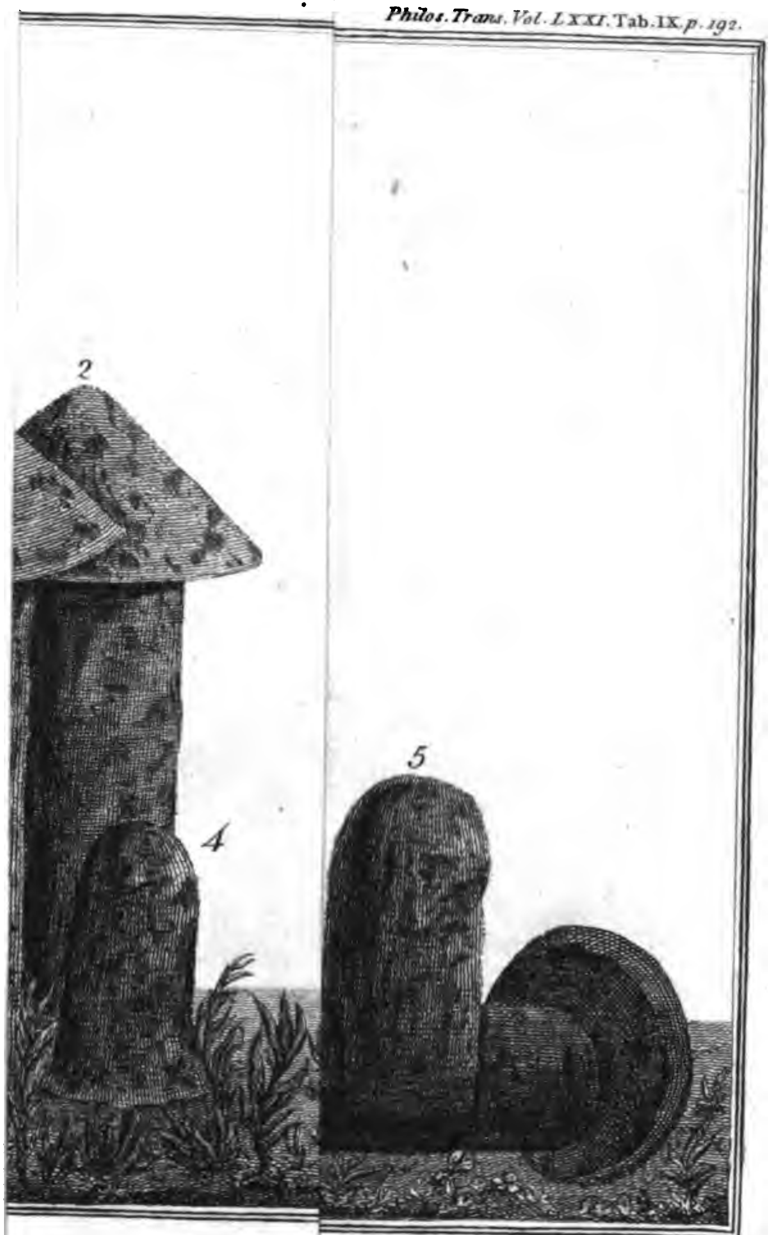

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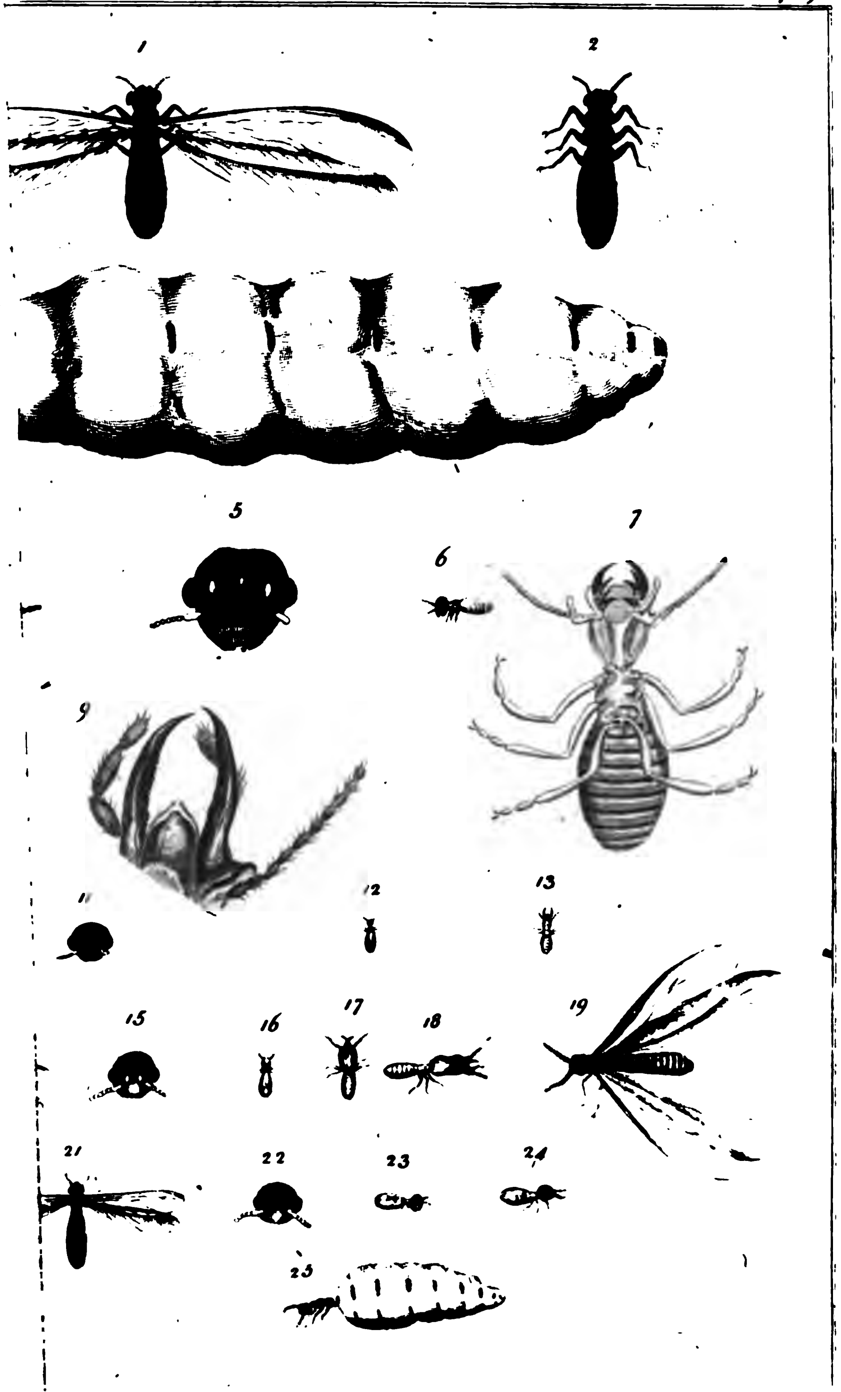
Philos. Trans. Vol. LXXI. Tab. X. p. 192.



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XII. *An Account of ſeveral Earthquakes felt in* Wales. *By* Thomas Pennant, *Eſq. F. R. S. in a Letter to Sir* Joſeph Banks, *P. R. S.*

Read January 25, 1781.

DEAR SIR,

Downing,
Dec. 12, 1781.

IT is very ſingular, that in three days after my return home I ſhould be reminded of my promiſe by a repetition of the very phenomenon on which I had engaged to write to you: for on Saturday laſt, between four and five in the evening, we were alarmed with two ſhocks of an earthquake; a ſlight one, immediately followed by another very violent. It ſeemed to come from the north-eaſt, and was preceded by the uſual noiſe; at preſent I cannot trace it farther than Holywell.

The earthquake preceding this was on the 29th of Auguſt laſt, about a quarter before nine in the morning. I was fore-warned of it by a rumbling noiſe not unlike the coming of a great waggon into my court-yard. Two ſhocks immediately followed, which were ſtrong enough to terrify us. They came from the north-weſt; were felt in Angleſea, at Caernarvon, Llanrwſt, in the iſle of Clwyd ſouth of Denbigh, at this houſe, and in Holywell; but I could not diſcover that their force extended any farther.

The next in this retrograde way of enumerating theſe phenomena was on the 8th of September 1775, about a quarter before ten at night, the noiſe was ſuch as preceded the former; and the ſhock ſo violent as to ſhake the bottles and glaſſes on the table round which myſelf and ſome company were ſitting. This ſeemed to come from the eaſt. I ſee in the Gentleman's Magazine of that year, that this ſhock extended to Shropſhire, and quite to Bath, and to Swanſea in South Wales.

The earlieſt earthquake I remember here was on the 10th of April 1750. It has the honour of being recorded in the Philoſophical Tranſactions, therefore I ſhall not trouble you with the repetition of what I have ſaid.

Permit me to obſerve, that I live near a mineral country, in a ſituation between lead mines and coal mines; in a ſort of neutral tract, about a mile diſtant from the firſt, and half a mile from the laſt. On the ſtricteſt inquiry I cannot diſcover that the miners or colliers were ever ſenſible of the ſhocks under ground: nor have they ever perceived, when the ſhocks in queſtion have happened, any falls of the looſe and ſhattery ſtrata, in which the laſt eſpecially work; yet, at the ſame time, the earthquakes have had violence ſufficient to terrify the inhabitants of the ſurface. Neither were theſe local; for, excepting the firſt, all may be traced to very remote parts. The weather was remarkably ſtill at the time of every earthquake I have felt.

I remain, with true regard, &c.

XIII. *Extract of a Letter from the Right Honourable* Philip *Earl* Stanhope, *F. R. S. to Mr.* James Clow, *Profeſſor of Philoſophy in the Univerſity of* Glaſgow. *Dated* Chevening, February 16, 1777.

Read June 10, 1780.

I HAVE lately made ſome curious obſervations concerning the roots of adfected equations, part of which have occurred to Meſſieurs DANIEL BERNOULLI, EULER, DE LA GRANGE, LAMBERT, and others; but ſome of them, I believe, are quite new. I will give you one inſtance of a quadratic equation, as the ſimpleſt.

Let the quadratic equation $11xx - 15x + 5 = 0$, be propoſed. I ſay then, that if two recurring ſeries be formed from the fractions $\frac{1+2z}{1-z-zz}$, $\frac{2+3z}{1-z-zz}$, which have a common denominator, and each ſeries of co-efficients, continued both ways (that is, as well before, as after the firſt term) the fractions formed by dividing each term of the firſt ſeries by the correſponding term of the ſecond ſeries, *viz.*

$$\&c.\ \frac{-11}{-14},\ \frac{+7}{+9},\ \frac{-4}{-5},\ \frac{+3}{+4},\ \frac{-1}{-1},\ \frac{-3}{-4},\ \frac{2}{3},\ \frac{1}{2},\ \Big|\ \frac{3}{5},\ \frac{4}{7},\ \frac{7}{12},\ \frac{11}{19},\ \frac{18}{31},\ \frac{29}{50}\ \&c.$$

will converge in the ſimpleſt manner poſſible; thoſe before the bar, in a retrograde order to the greater root $\frac{15+\surd 5}{22}$; and thoſe after the bar, in a direct order to the ſmalleſt root $\frac{15-\surd 5}{22}$; where it is to be obſerved, that the greater root is affirmative, notwithſtanding the ſign − being prefixed to ſome of the terms, becauſe in each fraction the numerator and the denominator are affected by the ſame ſign, whether + or −.

The chief improvement I have made conſiſts in approximating to two roots at once, by one and the ſame ſeries, continued backwards as well as forwards. I have not time to enlarge upon this ſubject at preſent; but the little I have ſaid will be a ſpecimen of the method to be uſed in higher equations.

XIV. *Extract of Two Meteorological Journals of the Weather obſerved at* Nain *in* 57° *North Latitude, and at* Okak *in* 57° 20′ *North Latitude, both on the Coaſt of* Labradore. *Communicated by Mr.* De la Trobe.

Read March 8, 1781.

THERMOMETER AT OKAK.

1779	Higheſt.	Loweſt.	Mean.
Auguſt	78,0	37,0	52,0
September	74,0	32,0	45,0
October	45,0	11,0	32,3
November	36,0	15,0	27,3
December	31,0	$\overline{12,0}$	12,7
1780			
January	35,0	$\overline{13,0}$	15,3
February	33,0	$\overline{17,0}$	13,1
March	45,0	$\overline{14,0}$	10,0
April	57,0	9,0	29,5
May	55,0	23,0	39,0
June	68,0	33,0	43,6
July	84,0	38,0	52,0

	Thermometer at Nain.			Barometer at Nain.		
1778	Higheſt	Loweſt	Mean.	Higheſt.	Loweſt.	Mean.
September	68,0	35,0	45,3	28,4½	27,10	28,1,3
October	60,0	15,0	35,1	28,8	27,4	28,2,0
November	37,0	$\overline{5,0}$	25,4	28,8	27,3	28,0,2
December	28,0	$\overline{30,0}$	1,9	28,7	27,2	28,2,4
1779						
January	13,0	$\overline{29,0}$	$\overline{14,3}$	28,8	26,11	28,1,4
February	13,0	$\overline{36,0}$	$\overline{13,2}$	28,5½	27,5	27,9,9
March	29,0	20,0	5,0	28,7	27,5	28,2,8
April	51,0	5,0	27,7	28,6½	27,3½	28,1,9
May	63,0	22,0	34,9	28,7½	27,9½	28,2,0
June	71,0	32,0	42,2	28,3	27,8	28,2,5
July	74,0	36,0	48,2	28,5	27,7	28,1,9
Auguſt	78,0	38,0	52,0	28,5	27,8	28,1,5
September	74,0	35,0	45.4	28,6	27,5	28,1,4
October	47,0	14,0	33,6	28,6	27,4½	28,2,5
November	36,0	9,0	27,3	28,5	27,2	28,6,1
December	32,0	$\overline{13,0}$	11,1	28,3½	27,1½	28,3,0
1780						
January	35,0	$\overline{16,0}$	13,4	28,2	26,6	27,6,8
February	32,0	$\overline{23,0}$	9,7	28,5½	26,10	27,9,5
March	39,0	$\overline{18,0}$	10,0	28,8	27,3	28,1,9
April	45,0	4,0	28,2	28,8½	27,7	28,3,7
May	53,0	23,0	37,6	28,6½	27,9	28,3,1
June	61,0	32,0	43,1	28,3	26,11	28,0,5
July	84,0	39,0	52,2	28,1½	27,5	28,1,3
Auguſt	70,0	37.0	50,4	28,3½	27,5	28,1,0

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METEOROLOGICAL JOURNAL

KEPT AT THE HOUSE OF

THE ROYAL SOCIETY

BY ORDER OF THE

PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL

for January 1780.

	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Jan. 1	8	0	27,0	33,0	30,24		NE	1	Foggy and froſty.
	2	0	31,0	34,0	30,17		SE	1	Foggy.
2	8	0	32,0	33,0	30,35		SE	1	Foggy.
	2	0	36,0	35,0	30,16		SE	1	Foggy.
3	8	0	41,0	37,0	30 09		SE	1	Foggy.
	2	0	43,5	38,5	30,08		SE	1	Foggy and cloudy.
4	8	0	35,0	38,0	30,26		N by W	1	Fair.
	2	0	39,0	39,5	30,34		NE	1	Fine.
5	8	0	31,0	36,0	30,30		WNW	1	Fair.
	2	0	36,0	37,5	30,28		NW	1	Fine.
6	8	0	33,5	35,0	30,00		W	1	Cloudy.
	2	0	37,5	36,5	29,98		SW	1	Foggy and rain.
7	8	0	36,0	36,5	30,14		ENE	1	Cloudy.
	2	0	36,5	38,5	30,20		NE	1	Fine.
8	8	0	26,0	33,0	30,15		NE	1	Fair and froſty.
	2	0	30,5	35,5	30,04		NE	1	Froſty.
9	8	0	22,5	30,0	29,95		NE	1	Froſty.
	2	0	29,5	31,0	29,88		E	1	Fine.
10	8	0	29,0	30,0	29,67		N by E	1	Cloudy.
	2	0	33,0	31,5	29,59		NE	1	Cloudy and ſnow.
11	8	0	25,0	29,5	29,58		SW	1	Cloudy.
	2	0	32,0	31,5	29,65		NNW	1	Foggy and ſnow.
12	8	0	24,5	29,5	29,61		NW	1	Fair and froſty.
	2	0	29,0	30,0	29,67		NW	1	Fine.
13	8	0	20,0	27,0	29,94		N by E	1	Fair and froſty.
	2	0	27,0	28,0	29,89		NE	1	Fair and froſty.
14	8	0	20,0	25,0	29,81		NE	1	Fair and froſty.
	2	0	30,0	28,0	29,89		NE	1	Fine.
15	8	0	21,5	24,5	29,45		NE	1	Fair and froſty.
	2	0	33,0	27,5	29,27		NE	1	Foggy.
16	8	0	40,0	32,0	28,70	0,185	SW	2	Fair.
	2	0	47,0	36 5	28,59		SW	2	Rainy.

METEOROLOGICAL JOURNAL

for January 1780.

	Time.	Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H. M.			Inches.	Inch.	Points.	Str.	
Jan. 17	8 0	40,0	37,0	28,73	0,035	SW	1	Cloudy.
	2 0	46,0	40,5	28,74		W by N	1	Fine.
18	8 0	36,5	38,0	28,92	0,042	NE	1	Cloudy.
	2 0	41,0	39,5	28,97		SW	1	Cloudy.
19	8 0	37,0	39,0	29,14	0,214	NE	1	Cloudy.
	2 0	40,0	37,5	29,28		NE	1	Cloudy.
20	8 0	34,0	38,0	29,37	0,150	N by E	1	Rain.
	2 0	35,5	39,0	29,43		NE	2	Rainy.
21	8 0	33,0	37,0	29,77	0,066	N by E	2	Fair.
	2 0	36,5	38,5	29,86		N by E	2	Fine.
22	8 0	25,0	33,0	30,00		N by W	1	Fair and froſty.
	2 0	32,0	35,0	30,00		N by E	1	Fine.
23	8 0	24,0	31,0	30,00		NE	1	Fair and froſty.
	2 0	28,0	32,5	30,05		NE	1	Foggy.
24	8 0	26,5	31,0	30,01		NW	1	Fair and froſty.
	2 0	34,0	32,5	29,99		NE	1	Fine.
25	8 0	29,0	30,0	30,12		N by E	1	Fair and froſty.
	2 0	35,5	33,5	30,06		N by E	1	Snow.
26	8 0	31,0	31,0	29,95		N by E	2	Cloudy.
	2 0	34,5	32,5	29,95		N by E	1	Cloudy and ſnow.
27	8 0	28,0	32,0	29,90		N	1	Cloudy with ſnow.
	2 0	31,0	32,5	29,92		N	1	Cloudy and ſnow.
28	8 0	30,0	32,5	29,92		E by N	1	Cloudy.
	2 0	32,5	34,0	29,90		E by N	1	Cloudy.
29	8 0	31,5	32,0	29,80		ENE	1	Cloudy.
	2 0	35,0	34,5	29,74		ENE	1	Cloudy.
30	8 0	29,0	31,0	29,66		NE	2	Cloudy.
	2 0	30,0	32,0	29,66		NE	2	Cloudy.
31	8 0	30,5	31,0	29,74		E by S	2	Cloudy.
	2 0	34,0	33,0	29,71		ENE	2	Cloudy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Feb. 1	8	0	30,5	32,0	29,65		NE	2	Cloudy.
	2	0	32,0	33,0	29,62		NE	1	Cloudy.
2	8	0	28,5	31,0	29,48		N by E	1	Fair.
	2	0	33,5	34,5	29,47		NE	1	Fair.
3	8	0	31,5	32,5	29,57		NW	1	Fair.
	2	0	36,5	34,5	29,69		NW	1	Cloudy.
4	8	0	29,0	33,0	29,74		SW	1	Fair.
	2	0	38,5	35,5	29,68		S by W	1	Fine.
5	8	0	30,5	33,0	29,43		NE	1	Fine.
	2	0	35,0	35,0	29,41		SE	1	Fine.
6	8	0	33,0	34,0	29,08	0,210	NE	1	Rainy.
	2	0	36,5	35,0	29,11		WSW	1	Rainy and foggy.
7	8	0	34,0	36,0	29,48	0,034	NW	1	Fair.
	2	0	38,5	38,0	29,68		NW	1	Fine.
8	8	0	32,5	36,0	29,89	0,022	SE	1	Fair.
	2	0	41,0	48,0	29,92		SE	1	Fine.
9	8	0	43,0	39,0	30,00	0,270	S by W	1	Cloudy.
	2	0	50,5	42,0	30,08		S by E	1	Cloudy.
10	8	0	45,5	44,5	30,22	0,015	SW	1	Cloudy.
	2	0	51,5	46,5	30,24		SW	1	Cloudy.
11	8	0	40,0	45,5	30,41		NE	1	Cloudy.
	2	0	44.0	45,5	30,45		ENE	1	Fine.
12	8	0	36,5	42,0	30,47		NE	1	Cloudy.
	2	0	41,0	43,0	30,47		ENE	1	Cloudy.
13	8	0	37,0	40,5	30,48		NE	1	Fair.
	2	0	41,5	42,0	30.52		NE	1	Fair.
14	8	0	34,0	39,5	30,50		N by W	1	Cloudy.
	2	0	43,0	41,5	30,46		N by W	1	Cloudy.
15	8	0	35,5	39,5	30,40		N by W	1	Fair.
	2	0	40,0	40,0	30,39		N by W	1	Cloudy.
16	8	0	37.0	39,0	30,08		NW	1	Fair.
	2	0	45,0	42,0	30,00		NW	1	Fair.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Feb. 17	8	0	32,0	38,0	30,19		S by E	1	Foggy.
	2	0	38,5	39,0	30,25		SE	1	Foggy and cloudy.
18	8	0	35,0	38,5	30,06		SW	1	Cloudy.
	2	0	41,0	41,0	29,92		SW	1	Cloudy.
19	8	0	36,0	40,5	30,41	0,030	NE	1	Fine.
	2	0	43,0	42,0	29,49		W	1	Fair.
20	8	0	27,5	37,0	29,56		NW	1	Fine.
	2	0	34,0	37,0	29,57		NNW	1	Snow.
21	8	0	27,5	33,0	29,72		NW	2	Fair.
	2	0	36,0	35,5	29,78		NW	2	Fine.
22	8	0	28,0	32,5	29,86		NW	2	Fine.
	2	0	30,0	37,5	29,94		NW	1	Fine.
23	8	0	28,0	32,5	30,18		NNW	1	Fine.
	2	0	38,0	35,0	30,24		NW	1	Fine.
24	8	0	34,0	34,0	30,23		SSW	1	Cloudy.
	2	0	41,0	36,0	30,27		SW	1	Rainy.
25	8	0	41,0	38,5	30,06	0,015	SW	2	Cloudy.
	2	0	47,0	49,0	29,88		SW	2	Cloudy.
26	8	0	26,5	35,0	29,83	0,128	NW	3	Fine.
	2	0	32,5	35,5	30,08		NE	3	Fine.
27	8	0	34,0	34,5	29,99		SW	2	Snowy.
	2	0	42,0	36,5	29,82		SSW	1	Cloudy.
28	8	0	42,0	40,0	29,92	0,034	WSW	1	Cloudy.
	2	0	53,5	44,5	29,95		WSW	2	Fine.
29	8	0	46,0	46,0	29,91		W by S	1	Fair.
	2	0	50,0	41,0	30,03		SW	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Mar. 1	7	0	34,5	43,0	30,10		W by S	1	Fair.
	2	0	47,0	46,0	30,09		SW	1	Fine.
2	7	0	45,0	45,5	29,78		SW	1	Cloudy.
	2	0	50,0	41,0	29,59		SE	1	Cloudy.
3	7	0	39,0	45,0	29,74		WNW	1	Cloudy.
	2	0	48,5	48,5	29,65		NW	1	Fine.
4	7	0	37,5	43,0	29,91		SW	1	Fair.
	2	0	50,5	46,0	30,05		W by N	1	Fine.
5	7	0	47,0	45,0	30,17	0,071	WSW	2	Fair.
	2	0	55,0	48,5	30,25		WSW	1	Cloudy.
6	7	0	45,0	47,5	30,37		SW	1	Fine.
	2	0	56,0	52,0	30,42		SW	2	Fine.
7	7	0	47,5	51,0	30,36		SW	1	Fair.
	2	0	57,0	53,0	30,39		SW	2	Fine.
8	7	0	48,0	51,0	30,37		SW	1	Cloudy.
	2	0	54,0	55,5	30,35		SW	1	Fair.
9	7	0	44,5	50,0	30,13		SSW	1	Cloudy.
	2	0	50,0	51,5	30,01		SW	1	Fine.
10	7	0	37,0	46,0	30,34		W by S	1	Fair.
	2	0	50,0	48,5	30,45		SW	1	Fine.
11	7	0	35,0	43,0	30,39		SE	1	Fair.
	2	0	50,0	47,0	30,25		SW	1	Fine.
12	7	0	44,0	45,0	29,78		S by W	1	Cloudy.
	2	0	52,0	48,5	29,79		SSW	1	Rainy.
13	7	0	35,0	44,0	30,16	0,072	SW	1	Fine.
	2	0	51,0	47,5	30,15		SW	1	Fine.
14	7	0	44,0	47,0	29,93		SSW	1	Fine.
	2	0	54,5	48,5	29,82		SW	2	Fine.
15	7	0	45,0	49,0	29,52	0,046	W by N	2	Rainy.
	2	0	48,5	50,0	29,58		WNW	2	Fine.
16	7	0	42,0	46,0	29,38	0,227	NW	1	Fine.
	2	0	51,0	50,0	29,67		NW	1	Fine.

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	Time.		Therm. without	Therm. within:	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Mar. 17	7	0	38,0	36,0	29,99		NW	1	Fine.
	2	0	49,5	56,0	29,42		NW	1	Fine.
18	7	0	50,0	51,0	29,67		NW	2	Cloudy.
	2	0	48,0	51,0	29,43		NW	2	Rainy.
19	7	0	40,0	58,0	29,35	0,360	NW	1	Fine.
	2	0	54,0	50,5	29,47		NW	1	Fine.
20	7	0	47,0	49,5	29,46	0,160	NE	1	Cloudy and rain.
	2	0	59,0	54,0	29,95		NW	1	Cloudy.
21	7	0	50,0	51,0	29,82		NW	1	Cloudy.
	2	0	58,0	54,0	29,78		NW	1	Cloudy.
22	7	0	44,0	50,0	29,83		NW	1	Cloudy.
	2	0	52,0	51,5	29,92		SW	2	Fine.
23	7	0	37,0	45,0	30,28		NW	1	Fine.
	2	0	56,0	46,0	30,27		SW	1	Fine.
24	7	0	35,5	43,5	30,11		E by S	1	Fair.
	2	0	51,5	46,5	30,00		E by N	1	Fine.
25	7	0	43,0	45,5	29,82	0,112	SSE	1	Fine.
	2	0	56,0	49,0	29,97		SE	1	Fine.
26	7	0	46,0	47,0	29,94		S by E	1	Fair.
	2	0	51,5	56,5	29,94		SSE	1	Fair.
27	7	0	52,5	51,5	29,90		SSE	1	Fair.
	2	0	62,5	55,0	29,85		SW	1	Fine.
28	7	0	51,0	54,0	29,62		E by N	1	Fair.
	2	0	56,0	56,5	29,61		SW	1	Rainy.
29	7	0	47,0	51,5	29,84	0,077	S by W	2	Fair.
	2	0	55,0	54,5	29,82		S by W	2	Fair.
30	7	0	51,5	53,5	29,71		SSW	1	Fair.
	2	0	55,0	55,5	29,69		SW	1	Showery with ſun-ſhine.
31	7	0	45,0	52,0	29,63	0,054	SSW	1	Fine.
	2	0	50,0	53,5	29,63		SW	1	Rainy.

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	Time.		Therm. without	Therm. within.	Barom	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Apr. 1	7	0	40,0	48,5	29,88	0,324	N by E	1	Fair.
	2	0	49,0	50,0	29,87		SSE	1	Fine.
2	7	0	44,0	46,0	29,51	0,014	SE	1	Cloudy.
	2	0	49,0	49,5	29,21		SSE	1	Rainy.
3	7	0	41,5	46,5	28,81	0,333	WSW	1	Cloudy.
	2	0	44,0	47,0	28,88		NNW	1	Rainy.
4	7	0	34,0	42,0	29,36	0,010	NNW	1	Fine.
	2	0	44,5	43,5	29,45		NW	2	Fine.
5	7	0	35,5	40,5	29,48	0,016	SW	1	Fine.
	2	0	35,5	41,0	29,50		NNW	1	Snow.
6	7	0	34,0	39,0	29,68	0,177	N by W	1	Fine.
	2	0	45,0	43,0	29,83		NW	1	Fine.
7	7	0	33,0	38,0	29,81	0,010	NE	1	Fair.
	2	0	46,5	42,0	29,72		SE	1	Fine.
8	7	0	34,0	39,0	29,71		NNE	1	Fine.
	2	0	46,5	42,5	29,80		N by W	1	Fair.
9	7	0	40,0	39,0	30,08	0,020	NNE	2	Fine.
	2	0	47,5	41,5	30,12		ENE	1	Fine.
10	7	0	33,5	38,0	30,17		SE	1	Fair.
	2	0	47,0	41,5	30,10		E by N	1	Fair.
11	7	0	41,0	41,0	29,77	0,050	S by E	1	Rainy.
	2	0	46,0	49,0	29,79		SE	1	Rainy.
12	7	0	42,0	43,5	29,50	0,156	NW	1	Fine.
	2	0	51,0	47,0	29,46		W by N	1	Cloudy.
13	7	0	38,0	43,0	29,57	0,250	NNE	2	Fine.
	2	0	37,0	44,5	29,69		NNE	2	Rain.
14	7	0	41,0	42,0	29,72	0,038	NW	2	Fair.
	2	0	46,5	44,5	29,75		NW	2	Fair.
15	7	0	43,5	43,5	29,75		WSW	1	Cloudy.
	2	0	51,5	46,0	29,74		W by N	1	Cloudy.
16	7	0	47,0	45,0	29,62	0,035	NW	2	Fine.
	2	0	55,0	49,0	29,67		WNW	2	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.		Points.	Str.	
Apr. 17	7	0	43,0	46,5	29,38	0,080	WNW	3	Fine.
	2	0	49,0	50,0	29,37		WNW	3	Fine.
18	7	0	41,5	46,5	29,50	0,056	SW	2	Cloudy.
	2	0	48 5	47,5	29,46		WSW	2	Showery.
19	7	0	43,0	44,0	29,72	0,050	N by E	2	Fine.
	2	0	53,0	48,0	29,76		N by E	2	Fair.
20	7	0	42,0	44,5	29,94	0,030	NW	1	Fine.
	2	0	52,0	48,0	30,01		NW	1	Fair.
21	7	0	47,5	48 0	29,91		SSE	1	Fair.
	2	0	52,0	50,0	29,83		SSW	2	Showery.
22	7	0	47,0	50,0	29,70	0,109	SW	1	Cloudy.
	2	0	58,0	53 5	29,71		SW	1	Fine.
23	7	0	47,5	50,0	29.52	0,031	S by W	1	Fine.
	2	0	56,5	54,0	29,47		SSW	2	Showery.
24	7	0	46,5	51,0	29,46	0,047	WSW	2	Fine.
	2	0	54,0	53,5	29,43		SSE	2	Showery.
25	7	0	45,0	50,5	29,51	0,053	S by E	1	Fine.
	2	0	56,5	54,0	29,46		SSW	1	Fine.
26	7	0	45,0	49,0	29,46	0,391	NW	2	Fine.
	2	0	58,0	53,5	29,74		NW	2	Fine.
27	7	0	50,0	50,5	29,50	0,210	SSW	1	Rainy.
	2	0	56,0	54,0	29,44		SSW	1	Rainy.
28	7	0	54,5	55,0	29,83	0,183	SW	1	Cloudy.
	2	0	60,5	59,5	29,85		SW	1	Rainy.
29	7	0	53,0	56,0	29,66	0,066	SW	1	Cloudy.
	2	0	65,5	60,5	29,66		SW	1	Fine.
30	7	0	55,0	58,0	29,71		SSW	1	Cloudy.
	2	0	61,0	65,0	29,73		S by W	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
May 1	7	0	53,0	58,0	29,76	0,067	SSW	1	Cloudy.
	2	0	65,0	62,0	29,79		SW	1	Fine.
2	7	0	52,0	56,0	29,85	0,243	N by W	1	Cloudy.
	2	0	59,5	59,0	30,04		NW	1	Fine.
3	7	0	45 5	51,0	30,26		E by S	1	Cloudy.
	2	0	57,0	55,0	30,26		SE	1	Fine.
4	7	0	48,0	52.5	30,18		WSW	1	Cloudy.
	2	0	64,0	56,5	30.16		SW	1	Fine.
5	7	0	53,0	55,0	29,92		SW	1	Cloudy.
	2	0	59,5	58.0	29,88		SW	1	Cloudy.
6	7	0	52,5	55,0	29,88		WSW	2	Fine.
	2	0	60,0	58,5	29,90		W by N	2	Cloudy.
7	7	0	53,0	55,0	29,94		W by S	1	Cloudy.
	2	0	63,5	59,5	29,94		SW	1	Fair.
8	7	0	52,0	57.0	29.72	0,032	SW	1	Rainy.
	2	0	55,0	57,5	29.58		S by W	2	Rainy.
9	7	0	540	55,0	29,67	0,091	SW	1	Fine.
	2	0	57,0	58,5	29,64		SW	2	Rainy.
10	7	0	53,0	54,5	29,38	0,200	SW	2	Fine.
	2	0	59,0	58,5	29.51		SW	2	Cloudy.
11	7	0	53 0	57.5	29 72		SW	2	Fine.
	2	0	58,0	59,0	29 88		SW	2	Rainy.
12	7	0	50,0	57,0	29,98		SSW	1	Fine.
	2	0	64,0	60,0	29,98		SW	1	Fine.
13	7	0	53,0	55,0	29,75	0,104	SW	1	Fair.
	2	0	63,0	60,5	29 73		SW	2	Fine.
14	7	0	51,5	57,0	29,78		W by S	1	Fine.
	2	0	57,0	58,5	29,71		SSW	1	Fine.
15	7	0	50,0	55,0	29,82	0,015	NW	2	Fine.
	2	0	33,0	68 5	29,94		NW	1	Fine.
16	7	0	54,5	56,5	30,12		NW	1	Fine.
	2	0	66,0	61,0	30,18		SW	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points	Str.	
May 17	7	0	58,5	61,0	30,14		SW	1	Fair.
	2	0	68,0	63,5	30,15		SW	1	Fine.
18	7	0	54,5	60,0	30,15		NW	1	Fine.
	2	0	67,0	62,5	30,17		NW	1	Fine.
19	7	0	57,5	61,5	29,92		SW	1	Cloudy.
	2	0	66,0	62,0	29,90		SW	1	Fair.
20	7	0	46,5	61,0	30,25		NW	1	Fair.
	2	0	61,0	58,5	30,26		NW	1	Fine.
21	7	0	57,0	53,5	29,49		SE	1	Fair.
	2	0	67,0	61,0	30,01		SW	1	Fine.
22	7	0	56,0	58,0	29,61	0,027	SSW	1	Rainy.
	2	0	65,0	61,0	29,60		SW	1	Fair.
23	7	0	55,5	58,5	29,67		SW	2	Cloudy.
	2	0	63,0	65,0	29,72		SW	2	Fine.
24	7	0	53,0	58,5	29,73	0,011	NW	1	Fair.
	2	0	63.5	60,5	30,23		SE	1	Fair.
25	7	0	57,0	59,5	30,21		SW	1	Cloudy.
	2	0	66,5	61,5	30,12		SW	1	Fine.
26	7	0	58,0	61,5	30,14	0,018	SW	1	Rainy.
	2	0	70,0	65,0	30,14		SW	1	Fine.
27	7	0	60,5	64,0	30,28		SW	1	Fine.
	2	0	73,5	70,0	30,22		SW	1	Fine.
28	7	0	68,0	66,5	30,17		SSW	1	Fine.
	2	0	78,5	72,5	30,18		SW	1	Fine.
29	7	0	68,0	71,0	30,06		SE	1	Fine.
	2	0	84,5	74,5	30,00		SSW	1	Fine.
30	7	0	65,0	67,0	29,98		SW	1	Fair.
	2	0	71,5	71,0	30,01		NW	2	Fine.
31	7	0	56,0	59,0	30,06	0,024	NE	1	Fair.
	2	0	66 0	66,0	30,14		NE	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
June 1	7	0	60,0	61,0	30,05		E by N	1	Fine.
	2	0	70,5	65,0	29,98		ENE	2	Fine.
2	7	0	66,5	67,0	29,93		SW	1	Fine.
	2	0	85,0	73,5	29,93		SW	1	Fine.
3	7	0	65,0	68,0	29,88		NE	1	Cloudy.
	2	0	71,0	72,0	29,95		NE	1	Fine.
4	7	0	59,0	66,0	30,07		NE	1	Fine.
	2	0	64,0	66,0	30,07		NE	1	Fair.
5	7	0	48,5	57,0	29,99	0,023	NE	1	Cloudy.
	2	0	55,5	59,0	29,99		NW	2	Cloudy.
6	7	0	49,0	55,0	29,99	0,010	NE	2	Cloudy.
	2	0	61,0	58,0	29,99		NE	2	Cloudy.
7	7	0	52,0	53,0	30,01		NE	1	Fair.
	2	0	60,0	58,0	30,02		NE	1	Fair.
8	7	0	48,0	50,0	29,99		NW	1	Fine.
	2	0	59,0	57,0	30,09		NW	1	Fine.
9	7	0	60,0	56,5	29,94		NW	1	Fine.
	2	0	62,5	60,0	29,94		NW	1	Cloudy.
10	7	0	61,0	59,0	29,80	0,018	SW	1	Fine.
	2	0	66,0	63,0	29,83		SW	1	Fair.
11	7	0	62,0	62,5	29,94		NW	1	Fine.
	2	0	70,0	65,0	29,90		W by N	1	Fine.
12	7	0	59,0	63,5	30,04		ENE	1	Cloudy.
	2	0	65,0	65,0	30,09		NE	1	Cloudy.
13	7	0	59,0	62,0	30,03		SW	1	Fine.
	2	0	72,0	69,5	29,98		SW	1	Fine.
14	7	0	55,5	61,5	29,81		SW	2	Fine.
	2	0	66,0	64,0	29,83		SW	2	Fine.
15	7	0	55,5	60,5	29,85	0,017	NW	1	Fine.
	2	0	63,0	64,0	29,92		NW	2	Cloudy.
16	7	0	56,0	59,0	30,01	0,020	WSW	1	Fair.
	2	0	63,0	62,0	30,04		SW	1	Fair.

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	Time.		Therm without	Therm: within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
June 17	7	0	58,5	60,5	29,91	0,030	SW	1	Fair.
	2	0	67,0	63,6	29,91		SW	1	Fair.
18	7	0	56,5	58,5	29,81	0,240	SW	1	Rainy.
	2	0	65,5	64,0	29,81		SW	1	Rainy.
19	7	0	60,0	62,5	29,79	0,152	SSW	1	Cloudy.
	2	0	72,0	67,0	29,79		SW	1	Fair.
20	7	0	60,0	61,0	29,95		W by S	1	Fine.
	2	0	66,0	69,0	29,99		SW	1	Fine.
21	7	0	57,5	62,0	30,07	0,022	W by N	1	Fine.
	2	0	68,0	64,5	30,11		SW	1	Fine.
22	7	0	58,5	62,5	30,11		SSW	1	Cloudy.
	2	0	61,0	64,0	30,07		SW	1	Rainy.
23	7	0	56,0	61,0	30,04	0,320	NW	1	Fine.
	2	0	70,0	66,0	30,18		SW	1	Fine.
24	7	0	60,0	63,5	30,10		SSW	1	Fair.
	2	0	67,5	66,5	30,07		SW	1	Fair.
25	7	0	58,0	65,0	30,08		W by S	2	Fair.
	2	0	67,0	65,0	30,12		NW	2	Fine.
26	7	0	57,5	62,0	30,24		NW	2	Fine.
	2	0	66,0	65,0	30,31		SW	1	Fine.
27	7	0	56,0	62,0	30,37		NW	1	Fine.
	2	0	69,0	64,5	30,37		SW	1	Fine.
28	7	0	62,0	64,0	30,32		NW	1	Fair.
	2	0	74,0	68,0	30,28		NNW	1	Fine.
29	7	0	66,0	67,5	30,25		SE	1	Fine.
	2	0	78,0	70,0	30,19		E	1	Fine.
30	7	0	67,0	66,0	29,99		SE	1	Fine.
	2	0	84,5	76,0	29,94		ESE	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
July 1	7	0	64,5	70,0	29.96		SW	1	Fair.
	2	0	73,0	73,5	29.95		S by E	1	Fair.
2	7	0	63,0	68 0	29 89	0,143	SW	1	Rainy.
	2	0	73 0	72,0	29,99		SW	1	Fine.
3	7	0	61,5	68,5	29,99		SW	1	Fine.
	2	0	69,0	70,0	30,05		SW	1	Cloudy.
4	7	0	60,0	66,5	30.02		SW	1	Fair.
	2	0	70,5	68,5	30 05		NW	1	Fine.
5	7	0	61,0	67,0	30,18		SW	1	Fine.
	2	0	69,0	66,0	30,09		SW	1	Fine.
6	7	0	61,5	64,0	30 09		NW	1	Fine.
	2	0	71,0	69,0	30,08		NW	1	Fine.
7	7	0	58,5	61,0	30,06		NW	1	Fine.
	2	0	65,0	64,5	30,04		NW	1	Fine.
8	7	0	60,5	62,5	29 97		W by N	1	Fine.
	2	0	68,5	65,5	29,99		NW	1	Fine.
9	7	0	61,0	62,0	30,06		NE	1	Fair.
	2	0	68,0	66,0	30,08		WNW	1	Fine.
10	7	0	57,0	64,0	30,14	0,017	NE	1	Fine.
	2	0	67,0	66,5	30,19		NNE	1	Fine.
11	7	0	54,0	62,0	30,17		N by E	1	Fair.
	2	0	70,5	68,5	30,14		NW	1	Fine.
12	7	0	61,0	65,0	29 97		WSW	1	Cloudy.
	2	0	74,0	70,0	29,94		NNW	1	Cloudy.
13	7	0	60,0	65,5	29 98	0,116	NNW	1	Cloudy.
	2	0	68,0	68,0	30,07		NW	1	Cloudy.
14	7	0	60,0	62,5	30,16		NW	1	Fine.
	2	0	70,5	68,5	30,19		NW	1	Fine.
15	7	0	60 5	66,5	30,00		WSW	1	Cloudy.
	2	0	72,0	69,5	29,92		NW	1	Fine.
16	7	0	66,0	69,5	29,99		NW	1	Fine.
	2	0	79,5	72,0	29 98		NW	1	Fine.

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	Time.		Therm without	Therm. within	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
July 17	7	0	68,5	73,0	29,88		SE	1	Fine.
	2	0	82,0	78,5	29,87		SE	1	Fine.
18	7	0	70,0	75,0	29,74	0,028	SW	1	Fine.
	2	0	72,0	76,5	29,83		SW	1	Fair.
19	7	0	62,0	71,0	30,00		WSW	1	Fair.
	2	0	68,5	69,0	30,06		SW	1	Fine.
20	7	0	58,5	67,0	30,13		WSW	1	Fine.
	2	0	72,5	69,5	30,09		WSW	1	Fine.
21	7	0	60,0	67,0	29,87	0,038	SW	1	Rainy.
	2	0	67,5	68,5	29,76		WSW	1	Cloudy.
22	7	0	61,0	67,0	29,59	0,926	SE	1	Rainy.
	2	0	66,0	68,5	29,66		S by W	1	Rainy.
23	7	0	59,5	66,0	29,99	0,319	NE	1	Cloudy.
	2	0	66,0	67,0	30,01		NE	1	Fair.
24	7	0	58,5	65,0	30,18		SW	1	Fair.
	2	0	79,0	69,5	30,17		SW	1	Fine.
25	7	0	65,0	69,5	30,16	0,024	NNE	1	Cloudy.
	2	0	80,0	72,0	30 19		NNE	1	Fine.
26	7	0	62,5	63,5	30,24		NW	1	Cloudy.
	2	0	78,0	75,0	30,23		SW	1	Fine.
27	7	0	64,0	70,5	30,14		NW	1	Fair.
	2	0	71,0	71,0	30,21		NW	1	Fine.
28	7	0	59,0	60,0	30,34		SSW	1	Fine.
	2	0	71,5	68,5	30,34		SW	1	Fine.
29	7	0	60,0	66,0	30,25		NE	1	Fine.
	2	0	71,0	68,0	30,19		E by S	1	Fine.
30	7	0	61,5	63,0	30,11		NE	1	Cloudy.
	2	0	75,0	70,0	30,11		ESE	1	Fine.
31	7	0	60,5	66,5	30,19		ENE	1	Cloudy.
	2	0	70,5	68,5	30,19		NE	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Aug. 1	7	0	59,0	65,0	30,20		NE	1	Cloudy.
	2	0	78,0	74,0	30,19		NE	1	Fine.
2	7	0	62,0	68,0	30,11		SW	1	Fair.
	2	0	78,0	71,0	30,18		SW	1	Fine.
3	7	0	61,5	68,5	30,09		NE	1	Fine.
	2	0	83,0	73,5	30,13		NE	1	Fine.
4	7	0	68,0	69,0	30,10		ENE	1	Fine.
	2	0	78,0	73,0	30,14		NE	1	Fine.
5	7	0	60,5	66,5	30,11		NNE	1	Fair.
	2	0	76,5	74,0	30,11		NW	1	Fine.
6	7	0	60,0	67,0	30,07		NNE	1	Cloudy.
	2	0	78,5	73,0	30,07		N by W	1	Fine.
7	7	0	62,0	69,0	30,12		NE	1	Fair.
	2	0	80,0	74,0	30,16		NE	1	Fine.
8	7	0	59.5	67,0	30,09		NE	1	Fair.
	2	0	77,0	75.0	30,07		NE	1	Cloudy.
9	7	0	62,5	66,0	30,02		NE	1	Fair.
	2	0	82,0	75.0	30,04		NE	1	Fine.
10	7	0	64.5	71,0	30,02		NE	1	Fine.
	2	0	81,5	76,5	30,02		E by S	1	Fine.
11	7	0	66,5	73,0	30,02		ENE	1	Fine.
	2	0	79,0	76,0	29,98		ENE	1	Fine.
12	7	0	65,0	72,0	29,91		NE	1	Fine.
	2	0	79.0	73,0	29,89		S by E	1	Fine.
13	7	0	62.5	65,0	29,89		NE	1	Fine.
	2	0	76,5	75,0	29,93		NE	1	Fine.
14	7	0	60,0	62,5	29,93		NE	1	Fair.
	2	0	62,0	69,0	29,95		NE	1	Rainy.
15	7	0	61,5	66,5	29.88	0,104	NE	1	Fair.
	2	0	68,0	69,0	29,92		NE	1	Fair.
16	7	0	65,0	68,0	29,91		NE	1	Fair.
	2	0	75,0	72,0	30,03		NE	1	Cloudy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.		Points.	Str.	
Aug. 17	7	0	65,5	67,5	30,02		SE	1	Fair.
	2	0	75,0	72,0	30,00		NE	1	Fair.
18	7	0	60,5	68 0	29,86	0,381	ENE	1	Rainy.
	2	0	71,0	71,0	29,85		ENE	1	Rainy.
19	7	0	64,0	66 5	29,92		NNE	1	Cloudy.
	2	0	74,0	72,5	29,99		NE	1	Fine.
20	7	0	63,5	66,0	30,12		ESE	1	Fine.
	2	0	74,5	71,5	30,13		SSE	1	Fine.
21	7	0	67,0	68,0	30,09		S by E	1	Fine.
	2	0	76,0	75,0	30,09		SE	1	Fine.
22	7	0	64,5	68,5	29,99		WSW	1	Fair.
	2	0	70,0	69,0	30,03		SW	1	Fine.
23	7	0	59,0	67,0	30,07		NW	1	Fair.
	2	0	72,0	70,0	30,14		SW	1	Fine.
24	7	0	58,5	63,0	30,18		ENE	1	Fair.
	2	0	74,0	72,5	30,19		NW	1	Fine.
25	7	0	59,5	62,0	30,13		NNE	1	Fine.
	2	0	75,0	73,0	30,15		NW	1	Fine.
26	7	0	60,5	64,0	30,16		NE	1	Fine.
	2	0	76,5	75,0	30,16		ENE	1	Fine.
27	7	0	61,0	63,0	30,15		E by N	1	Fine.
	2	0	73,5	69,5	30,08		E by N	1	Fine.
28	7	0	60,0	65,0	30,10		NE	1	Fair.
	2	0	68,5	68,0	30,11		NE	1	Fine.
29	7	0	61,0	65,5	30,19		NE	1	Fair.
	2	0	72,0	71,0	30,14		NE	1	Fine
30	7	0	61,0	67,0	30,10		NE	1	Cloudy.
	2	0	76,5	74,0	30,09		NE	1	Fine.
31	7	0	69,0	69,5	30,05		SSW	1	Fine.
	2	0	83,5	76,5	30,05		S by W	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Sept. 1	7	0	67,5	73,5	30,01		SSW	1	Fine.
	2	0	84,0	79,0	29,89		SW	1	Fine.
2	7	0	66,0	73,0	29,95		W by S	1	Fair.
	2	0	77,0	76,0	29,94		SW	1	Fine.
3	7	0	63,0	71,0	30,01		NE	1	Fine.
	2	0	70,0	71,0	30,05		SW	1	Fine.
4	7	0	56,0	63,5	30,01		NE	1	Fair.
	2	0	70,0	68,0	30,17		SW	1	Fair.
5	7	0	53,5	62,5	30,20		E by S	1	Fine.
	2	0	71,5	69,0	30,18		SE	1	Fine.
6	7	0	56,0	63,0	30,02		SE	1	Fine.
	2	0	71,5	69,0	29,95		SE	1	Fine.
7	7	0	57,5	62,0	29,75		ENE	1	Cloudy.
	2	0	76,0	69,0	29,73		SE	1	Cloudy.
8	7	0	65,0	68,0	29,59	0,250	SSW	1	Fair.
	2	0	72,0	70,5	29,61		SE	1	Fine.
9	7	0	58,0	67,5	29,74		S by W	1	Rainy.
	2	0	64,0	68,0	29,81		SW	1	Cloudy.
10	7	0	54,0	64,5	29,83	0 560	NNE	1	Cloudy.
	2	0	70,0	67,0	29,74		S by E	1	Rainy.
11	7	0	59,0	65,0	29,71	0,327	SSW	1	Rainy.
	2	0	67,0	67,0	29,82		SW	1	Cloudy and rain.
12	7	0	54,0	63,0	29,96	0,258	SW	1	Fine.
	2	0	67,5	66,5	29,99		NW	1	Fine.
13	7	0	61,0	62,0	30,03		S by W	1	Fine.
	2	0	69,0	65,5	29,99		SSW	1	Fair.
14	7	0	57,0	64,0	29,85	0,167	SE	1	Rainy.
	2	0	66,0	66,5	29,86		S by E	1	Cloudy.
15	7	0	57,0	63,5	29,92		S by E	1	Fine.
	2	0	62,0	73,5	29,98		SE	1	Fine.
16	7	0	62,5	65,0	29,76		SE	1	Showery.
	2	0	59,0	67,5	29,76		SE	1	Showery.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.		Points.	Str.	
Sept. 17	7	0	60,5	65,0	29,49	0,225	SE	1	Rainy.
	2	0	68,0	67,0	29,49		SE	2	Fine.
18	7	0	58.0	63,5	29,49	0,572	SSW	1	Fair.
	2	0	68,0	67,0	29,58		SW	1	Fine.
19	7	0	53,0	60,0	29,64		SSE	1	Fine.
	2	0	65,0	65,0	29,68		SE	1	Fine.
20	7	0	52,5	60,5	29,67		SE	1	Fine.
	2	0	64,0	62,5	29,66		NW	1	Fine.
21	7	0	49,5	59,5	29,81		S by W	1	Fine.
	2	0	65,0	62,0	29,88		SW	1	Fine.
22	7	0	48,0	56,0	30,02		SSW	1	Fine.
	2	0	65,0	65,0	30,11		SW	1	Fine.
23	7	0	49,5	57,0	30,09		ENE	1	Fine.
	2	0	66,0	66.0	30,05		NE	1	Fine.
24	7	0	59,0	59,5	29,84		NW	1	Fair.
	2	0	66,5	65,0	29,99		NW	1	Showery.
25	7	0	54,0	58,0	30,04	0,113	SSW	1	Fine.
	2	0	66,5	67,0	30,04		WSW	1	Cloudy.
26	7	0	59,0	61,0	29,97		SW	1	Fair.
	2	0	67,0	64,0	29,92		SW	1	Fair.
27	7	0	51,0	59,0	29,90	0,079	WSW	1	Fine.
	2	0	66,0	63,5	29,94		SW	1	Fair.
28	7	0	56,0	60,0	29,75		SW	1	Cloudy.
	2	0	64,0	64,5	29,76		SW	1	Showery and cloudy.
29	7	0	53,5	61,5	29,54	0,320	SSE	1	Rainy.
	2	0	62,0	63,0	29,64		SE	1	Fair.
30	7	0	50,5	57,0	29,34	0,032	SSW	1	Rainy.
	2	0	52,0	57,0	29,14		SW	2	Rainy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Oct. 1	7	0	45,0	52,5	29,19	0,076	SSW	1	Rainy.
	2	0	58,5	55,5	29,09		SSW	1	Rainy.
2	7	0	53,0	55,0	29,07	0,106	SW	1	Fair.
	2	0	57,0	56,5	29,44		SW	2	Fine.
3	7	0	52,5	54,0	29,57		WSW	1	Fine.
	2	0	58,5	55,5	29,64		NW	1	Fine.
4	7	0	42,0	51,5	29,79	0,050	NNE	1	Fair.
	2	0	54,0	59,5	29,87		NW	1	Fair.
5	7	0	41,5	50,5	30,16		NW	1	Fine.
	2	0	54,0	52,5	30,18		WSW	1	Fair.
6	7	0	45,5	49,0	30,04		S by E	1	Cloudy.
	2	0	57,0	53,5	29,90		S by W	1	Cloudy.
7	7	0	52,0	53,0	29,63		SSW	1	Fair.
	2	0	58,5	55,5	29,56		SW	1	Fine.
8	7	0	43,5	52,0	29,62		SSW	1	Fair.
	2	0	57,5	54,5	29,44		SE	1	Fine.
9	7	0	56,5	55,0	28,66	0,590	SSW	3	Rainy.
	2	0	57,0	56,5	28,82		SSW	3	Showery.
10	7	0	51,5	50,0	28,68		SW	1	Fair.
	2	0	58,0	55,0	28,99		SW	2	Cloudy.
11	7	0	45,5	53,5	29,51	0,020	SW	2	Fair.
	2	0	58,0	56,0	29,61		SW	2	Fair.
12	7	0	43,5	53,0	29,68	0,025	SW	1	Fair.
	2	0	55,0	54,0	29,81		W by N	1	Fine.
13	7	0	48,5	51,5	30,04		SSE	1	Cloudy.
	2	0	60,0	65,0	30,02		SE	2	Fair.
14	7	0	52,0	53,5	29,75		SE	1	Cloudy.
	2	0	65,0	59,0	29,61		SE	1	Fine.
15	7	0	58,5	59,0	29,69		SSE	1	Cloudy.
	2	0	66,0	62,0	29,69		SE	1	Cloudy.
16	7	0	53,5	56,5	29,72	0,530	SSW	1	Rainy.
	2	0	62,0	61,0	29,71		WSW	1	Cloudy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Oct. 17	7	0	55,0	58,0	29,87		SSW	1	Fine.
	2	0	65,0	63,5	29,91		SSW	1	Fine.
18	7	0	49,5	55,0	30,02		NNE	1	Cloudy.
	2	0	62,0	60,0	30,07		NNE	1	Fine.
19	7	0	53,0	59,0	29,79		SW	2	Fine.
	2	0	56,0	59,0	29,58		SW	2	Rainy.
20	7	0	46,5	56,0	29,48	0,071	WSW	1	Fine.
	2	0	52,0	56,0	29,44		SW	1	Cloudy.
21	7	0	47,0	50,5	29,48		NW	2	Fine.
	2	0	55,0	53,0	29,63		WNW	1	Fine.
22	7	0	46,0	50,5	29,73		SW	1	Cloudy.
	2	0	54,5	52,5	29,67		SW	1	Cloudy and rainy.
23	7	0	41,5	49,0	29,86	0,068	SW	1	Fine.
	2	0	53,0	52,0	29,88		SW	2	Cloudy.
24	7	0	50,0	51,5	29,66		SE	1	Cloudy.
	2	0	59,0	61,0	29,70		SE	1	Fair.
25	7	0	43,5	51,0	29,73		NE	1	Fine.
	2	0	51,5	52,0	29,77		NW	1	Fine.
26	7	0	37,0	48,0	30,07		SSW	1	Fine.
	2	0	52,0	50,0	30,14		NNE	1	Fine.
27	7	0	45,0	46,0	30,27		NE	1	Cloudy.
	2	0	50,0	48,0	30,29		NE	1	Fair.
28	7	0	47,0	48,0	30,14		NNE	1	Cloudy.
	2	0	55,0	51,5	30,07		NE	1	Cloudy.
29	7	0	47,5	51,0	29,85		NW	1	Rainy.
	2	0	49,5	51,5	29,82		NW	1	Rainy.
30	7	0	49,5	51,5	29,71	0,435	NNW	1	Rainy.
	2	0	50,0	52,0	29,82		NNW	1	Rainy.
31	7	0	49,5	52,0	29,88	0,375	SW	1	Rainy.
	2	0	50,5	52,5	29,89		NE	1	Rainy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Nov. 1	8	0	47,5	49,5	29,94	0,118	NNE	1	Cloudy.
	2	0	50,0	51,0	30,02		NE	2	Rainy.
2	8	0	46,0	48,5	30,08		NNE	1	Cloudy.
	2	0	50,0	51,0	30,08		NE	1	Fine.
3	8	0	46,0	48,5	29,97		NE	1	Cloudy.
	2	0	50,0	53 0	29,94		NE	1	Fair.
4	8	0	40,0	46,0	29 97		NW	1	Fine.
	2	0	46,0	47,0	30,08		NW	2	Fine.
5	8	0	47,0	47,0	29,83	0,039	SW	1	Fine.
	2	0	51,5	49,0	29,72		NW	2	Fine.
6	8	0	40,0	45,0	29 86		NW	2	Fine.
	2	0	44,0	45,0	29 87		NW	2	Fine.
7	8	0	35,0	37,5	29 72	0,039	NNE	2	Fine.
	2	0	36,0	38 0	29 82		NE	3	Snowy.
8	8	0	36,0	37,5	29,91	0,081	NNE	2	Fine.
	2	0	42,0	40,0	29 94		NE	1	Fine.
9	8	0	33,0	37,5	30 09		SW	1	Cloudy.
	2	0	41,0	40,0	30,05		SSW	1	Cloudy.
10	8	0	48,5	44,0	30 07	0,075	NE	1	Cloudy.
	2	0	52,0	47,0	30,11		NE	1	Fair.
11	8	0	46,5	45 0	30 11		NE	1	Cloudy.
	2	0	52,0	48,0	30 17		NNE	1	Cloudy.
12	8	0	47,0	49,0	30,09		WSW	1	Fine.
	2	0	51,0	49,5	30,12		WSW	1	Cloudy.
13	8	0	51,5	50,0	29,73		SW	3	Cloudy.
	2	0	52,0	51,0	29,64		SW	3	Cloudy.
14	8	0	39,5	47,0	29 82		WSW	1	Fair.
	2	0	47,0	47 5	29,84		WSW	1	Fine.
15	8	0	30,0	41,0	29,97	0,095	NW	1	Fine.
	2	0	36 0	41 0	30,02		NW	1	Fine.
16	8	0	26,0	36 0	29,93		SE	1	Foggy.
	2	0	28,5	38,0	30 16		SE	1	Foggy.

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	Time.		Therm without	Therm within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Nov. 17	8	0	36,5	38,5	29,73		SE	1	Foggy.
	2	0	40,0	41,0	29,63		SE	1	Rainy.
18	8	0	41,0	40,0	29,60	0,030	SE	1	Cloudy.
	2	0	45,0	42,0	29,53		SSE	1	Fair.
19	8	0	41,0	42,5	29,27	0,039	NW	1	Fair.
	2	0	43,5	44,0	29,32		NW	1	Fine.
20	8	0	43,0	43,5	28,94	0,616	W by S	1	Cloudy.
	2	0	46,0	49,0	29,02		SW	1	Fair.
21	8	0	36,5	41,5	29,14	1,074	NW	1	Fair.
	2	0	41,5	43,0	29,31		NW	1	Cloudy.
22	8	0	36,0	41,0	29,14	0,025	SW	1	Cloudy.
	2	0	37,0	40,5	29,08		WNW	1	Fine.
23	8	0	32,0	37,0	29,39		NW	1	Fine.
	2	0	37,0	38,0	29,42		NW	1	Fair.
24	8	0	31,0	39,0	29,64		SW	1	Foggy.
	2	0	36,0	38,0	29,50		SW	1	Rainy.
25	8	0	36,0	39,0	29,89	0,236	NW	1	Cloudy.
	2	0	38,0	40,5	29,98		WSW	1	Cloudy.
26	8	0	42,0	39,5	30,10		WSW	1	Cloudy.
	2	0	48,0	42,0	30,14		NW	1	Cloudy.
27	8	0	44,0	45,0	30,27	0,038	NW	1.	Cloudy.
	2	0	47,0	45,0	30,28		NNW	1	Cloudy.
28	8	0	42,0	44,5	30,46		ENE	1	Cloudy.
	2	0	45,5	46,0	30,47		NE	1	Fine.
29	8	0	37,0	42,0	30,45		NNE	1	Fine.
	2	0	40,0	43,0	30,40		NNE	1	Fine.
30	8	0	37,5	42,5	30,32		NNE	1	Fine.
	2	0	40,5	42,0	30,32		NNE	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Dec. 1	8	0	38,5	40,5	30,13		NE	1	Fair.
	2	0	43,5	41,5	30,31		NE	1	Fair.
2	8	0	42,0	42,5	30,34		NE	1	Cloudy.
	2	0	44,0	46,0	30,36		NE	1	Cloudy.
3	8	0	38,0	40,0	30,47		NE	2	Cloudy.
	2	0	41,5	43,0	30,49		NE	2	Cloudy.
4	8	0	40,5	41,5	30,51		NE	1	Fine.
	2	0	45,0	44,0	30,49		NE	1	Cloudy.
5	8	0	39,5	42,0	30,45		NE	2	Cloudy.
	2	0	43,5	44,0	30,40		NE	1	Cloudy.
6	8	0	38,0	41,5	30,35		NE	1	Cloudy.
	2	0	39,0	42,5	30,33		NE	1	Cloudy.
7	8	0	33,0	39,5	30,29		NE	1	Cloudy.
	2	0	33,5	36,0	30,28		NE	1	Cloudy.
8	8	0	36,0	39,0	30,29		NE	1	Fine.
	2	0	40,0	40,0	30,31		NE	1	Fair.
9	8	0	45,0	48,0	30,53		NE	1	Fair.
	2	0	40,0	40,0	30,53		NE	1	Fair.
10	8	0	33,5	37,0	30,45		WSW	1	Fair.
	2	0	38,0	36,0	30,44		SW	1	Cloudy.
11	8	0	38,5	40,0	30,36		NW	1	Fine.
	2	0	45,0	42,0	30,34		NW	1	Fair.
12	8	0	41,0	41,5	30,45		SW	1	Foggy.
	2	0	45,5	43,0	30,41		S by E	1	Foggy.
13	8	0	40,5	43,5	30,44		NW	1	Foggy.
	2	0	44,0	44,0	30,39		WNW	1	Cloudy.
14	8	0	40,0	42,0	30,36		SW	1	Foggy.
	2	0	45,0	45,0	30,46		SW	1	Fair.
15	8	0	41,0	44,0	30,44		NW	1	Fine.
	2	0	45,0	45,0	30,46		NW	1	Fair.
16	8	0	40,0	44,5	30,55		NNE	1	Foggy.
	2	0	43,5	45,0	30,55		SE	1	Cloudy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Dec. 17	8	0	40,0	44,0	30,51		SW	1	Foggy.
	2	0	43,5	44,0	30,48		NW	1	Fine.
18	8	0	34,5	41,0	30,46		NE	1	Foggy.
	2	0	40,0	41,5	30,46		ENE	1	Fine.
19	8	0	30,5	38,0	30,49		ENE	1	Fine.
	2	0	34,5	36,0	30,40		NE	1	Fine.
20	8	0	30,0	34,5	30,08		NE	1	Foggy.
	2	0	32,0	39,0	29,99		NE	1	Foggy.
21	8	0	28,0	33,0	29,69		S by E	1	Foggy.
	2	0	29,0	34,5	29,69		S by E	1	Foggy.
22	8	0	33,0	34,0	29,85		WSW	1	Foggy.
	2	0	36,5	35,0	29,89		NW	1	Fine.
23	8	0	24,0	32,0	30,05		SSW	1	Foggy.
	2	0	32,5	34,0	30,06		NW	1	Fine.
24	8	0	34,0	34,5	30,15		NNE	1	Foggy.
	2	0	37,0	35,5	30,15		N by E	1	Fair.
25	8	0	37,0	37,5	30,21		NNE	1	Fair.
	2	0	38,5	37,5	30,23		NNE	1	Fair.
26	8	0	32,5	34,5	30,26		ENE	1	Fair.
	2	0	36,0	36,5	30,26		N by E	1	Fair.
27	8	0	29,5	32,5	30,26		NE	1	Froſty.
	2	0	32,0	34,5	30,29		NE	1	Froſty.
28	8	0	32,5	33,5	30,35	0,100	SSW	1	Foggy.
	2	0	37,0	36,5	30,34		SW	1	Cloudy.
29	8	0	40,5	37,0	30,13	0,210	SW	1	Cloudy.
	2	0	45,0	39,0	30,03		WSW	1	Fair.
30	8	0	45,0	41,0	30,01		WSW	1	Cloudy.
	2	0	50,0	44,0	30,07		SW	1	Cloudy.
31	8	0	43,5	44,0	30,06		SSW	1	Cloudy.
	2	0	50,0	46,5	30,07		WSW	1	Fair.

1780.

1780	Thermometer without.			Thermometer within.			Barometer.			Rain.
	Greateſt Height.	Leaſt Height.	Mean Height.	Greateſt Height.	Leaſt Height.	Mean Height.	Greateſt Height.	Leaſt Height.	Mean Height.	Inches.
Jan.	47,0	20,0	31,9	40,5	24,5	33,6	30,35	28,59	29,77	0,692
Feb.	53,5	20,0	37,8	49,0	31,0	38,4	30,52	29,08	29,91	0,858
Mar.	59,0	34,5	51,4	56,5	36,0	49,9	30,45	29,38	29,91	1,189
Apr.	65,5	33,0	46,7	65,0	38,0	48,0	30,17	28,81	29,65	2,739
May	84,5	45,0	59,7	74,5	51,0	60,4	30,28	29,38	29,94	0,822
June	84,5	48,0	62,9	76,0	50,0	63,2	30,37	29,79	30,01	0,852
July	82,0	54,0	66,4	78,5	61,9	69,4	30,34	29,59	30,05	1,602
Aug.	83,5	58,5	69,2	76,5	62,0	68,3	30,20	29,85	30,05	0,485
Sept	84,0	48,0	61,3	79,0	56,0	65,2	30,20	29,14	29,83	2,903
Oct.	66,0	41,5	52,3	65,0	46,0	53,9	30,29	28,66	29,69	2,356
Nov.	52,0	26,0	42,0	53,0	36,0	43,6	30,47	28,94	29,85	2,505
Dec.	50,0	24,0	38,5	46,5	32,0	39,8	30,55	29,69	30,29	0,310
Whole year			51,7			52,8			29,91	17,313

VARIATION NEEDLE.

	7 h. A. M.	12 h. M.	2 h. P.M.	10 or 11h. P.M.	Daily Means.
	° ′	° ′	° ′	° ′	° ′
June 5	22 25	22 26	22 21	22 23	22 24
6	22 21	22 29	22 57	22 44	22 38
7	22 30	22 49	22 48	22 29	22 39
8	22 45	22 44	22 45	22 46	22 45
9	22 31	22 42	22 48	22 40	22 40
10	22 25	22 48	22 57	22 40	22 42
11	22 45	22 49	22 48	22 44	22 46
12	22 37	22 49	22 46	22 41	22 43
13	22 31	22 53	22 45	22 35	22 41
14	22 29	22 55	22 50	22 50	22 46
15	22 25	22 40	22 49	22 47	22 40
16	22 45	22 48	22 47	22 49	22 47
17	22 27	22 32	22 55	22 55	22 42
18	22 39	22 37	22 41	22 48	22 41
Means	22 32	22 43	22 47	22 42	

Mean of all 22° 41′.

DIPPING NEEDLE.

	7 h. A.M.	12 h. M.	2 h. P.M.	10 or 11 h. P.M.	Means.	
	° ′	° ′	° ′	° ′	° ′	
June 5	72 0	72 5	72 0	72 0	72 3	Weſt mark down.
6	72 5	72 5	72 10	72 10		
7	72 0	72 0	72 10	72 0		
8	72 0	72 0	72 5	72 10	72 21	Eaſt.
9	72 20	72 20	72 20	72 20		
10	72 20	72 20	72 30	72 10		
11	72 20	72 30	72 25	72 10		
12	72 30	72 30	72 30	72 0	72 13	Eaſt mark uppermoſt.
13	72 5	72 15	72 10	72 10		
14	72 10	72 15	72 20	72 10		
15	72 20	72 15	72 20	72 20		
16	72 45	72 40	72 40	72 20	72 32	Weſt.
17	72 40	72 30	72 25	72 30		
18	72 40	72 20	72 30	72 20		

THE END OF PART I. OF VOL. LXXI.

ELOCUTION AND POLITE LITERATURE.

MR. HENRY SMEATHMAN, who was many years a pupil of the late Mr. Rice, and before he went on his travels, practiſed with ſucceſs that Author's ingenious method of teaching to read, ſpeak and write, with eaſe, energy and propriety; begs leave to offer his ſervices to the public in the ſame mode of uſeful and polite inſtruction.

Gentlemen intending themſelves for the Church, the Bar or the Senate, will by oral and familiar converſation, in a few months, receive more improvement, than by a ſolitary ſtudy of many years, in an art not taught in our Schools or Univerſities, yet abſolutely neceſſary to thoſe who wiſh to excel in public or polite life, or aſpire to eminence in eloquence and literature.

By this art, a liberal education is improved and accompliſhed, claſſical learning advantageouſly diſplayed, and the want of a proficiency in the dead and other languages ſupplied, as by Mr. Smeathman's method, founded on that of Mr. RICE, the pupils may acquire a critical knowledge of the idiom and elegance of their own without them.

This recommends it peculiarly to the attention of all thoſe who wiſh to have the pleaſure of reading the Engliſh claſſic authors with grace and propriety, or to write and ſpeak correctly; of thoſe Gentlemen, who are intended for the Navy, the Army, the Public-office or the Counting-houſe; or whoſe juvenile ſtudies may have been obliterated, by an early introduction into thoſe ſituations; and of all thoſe who may have been deprived of the advantages of a liberal education.

They are effectually cured of any defects in pronunciation, which do not ariſe from an imperfection in the organs of ſpeech; and inſtead of provincial, diſagreeable or abſurd habits, they acquire an energetic, a perſuaſive and a graceful manner of ſpeaking, reading and reciting. Above all, the younger pupils receive this ſingular and important advantage, that their minds are opened more early, their reaſon is aſſiſted in its exertions, and directed to ſuch objects as naturally promote the perfection, and the happineſs of human beings.

This method is equally well adapted to both ſexes, thoſe advanced in life may receive inſtruction privately, as uſual, and Ladies will be attended at their own houſes.

Application may be made to Mr. Smeathman, at his Chambers, No 14. Clement's-Inn. Of whom, in a few days, may be had a ſhort Treatiſe on the nature of eloquence, and the importance of learning to read, ſpeak and write our own language, independent of all others; with a plan of his mode of teaching, and the advantages to be derived from it.

The following are detached extracts from the above-mentioned Treatiſe.

"Thoſe who have not ſtudied and been exerciſed in reading, "ſpeaking or compoſing, in their own language, will not do "either in ſuch a manner as to claim or deſerve attention; "and it is the talent of performing them with eaſe, energy "and propriety, which we call eloquence.

"People are uſually very ſolicitous to have their children in"ſtructed in all thoſe liberal arts, which are deemed requiſite to "perfect them as Gentlemen and Scholars: and ſurely as no"thing contributes more to this intention, they would not omit "oratory, if they thought it was to be learned. They liſten "with rapture to the charms of eloquence, but are not aware "that it is in their power to procure this ineſtimable talent "for thoſe who are the objects of their inceſſant care and daily "anxiety."——

—"This can only ariſe from the erroneous idea that oratory is "not to be taught or acquired. Yet no art is more ſimple in "its Principles, or clearer in its rules, and no rules are more "eaſily reduced to practice, or more certainly practiſed with "ſucceſs."——

—"It is granted that people may improve themſelves much "by private declamation. The art of eloquence, however, "is in this reſpect like fencing, in which a man can learn "very little by puſhing againſt a wall: he may indeed "ſtrengthen his muſcles, and enable himſelf to bear the "fatigue, but it is by frequent exertion with a maſter only "that he can expect to acquire ſkill.

" It is from the friendly obſervations, the oral information, " the frequent contentions and the unreſtrained corrections, " of a ſkilful companion, that a ſtudent in oratory can derive " eſſential improvement: writing alone can no more commu- " nicate the art than it can ſpeech to the dumb.

" Thoſe therefore who educate their children for eminent " ſtations in life, with the hope that they will one day render " themſelves conſpicuous in the Senate, the Church or the " courts of Law and Juſtice, without having them grounded in " the firſt principles of eloquence, expect to reap where they " have not ſown: they muſt in all probability be diſappoint- " ed."———

—" There is ſcarce a man of a moderate age and capacity not " born with ſome invincible imperfection in the organs of " ſpeech, who may not in twelve months be enabled to com- " municate his thoughts with pleaſure to his audience, and " credit to himſelf; or ſo to read the Liturgy as to excite " Devotion in the moſt lukewarm, and to confirm it by a na- " tural, perſuaſive and convincing manner of delivering the " great and eſſential truths of Religion; and how much this " is to be wiſhed is left to the opinion of all who have heard " the diſguſting and unedifying manner in which the fineſt " compoſitions are generally delivered in our public places of " worſhip, and ſeen the awkward figures ſome of our learned " Senators make when they attempt to ſpeak in Parliament."—

—" We have hitherto treated only of the moſt obvious advan- " tages ariſing from the ſtudy of eloquence. On a cloſer

" view, many other important benefits will be found to be " derived from it. Of theſe, opening the mind, improving " the heart and infuſing a deſire of knowledge, deſerve par- " ticular notice. Such is the tendency, effect and benefit, of " the mode of inſtruction here recommended; for as it applies " directly to the underſtanding without burthening the me- " mory, or perplexing the mind, the youngeſt pupils recur " to it with delight; and its uncommon ſucceſs, when at- " tempted, has ariſen from the immediate conviction which " they feel, that our path to knowledge is pleaſant, and " the poſſeſſion of it highly advantageous and deſirable."—

" Our youth are generally very defective in thoſe polite " Branches of Knowledge without which claſſical Learning is " of little uſe. Their manners frequently continue rough and " puerile, when they ought to be graceful and manly, their " ſpeech inarticulate or ſlovenly, diſſonant or defective, diſ- " agreeable, and often unintelligible; and very few, even " among thoſe who are good ſcholars, can read or write their " own language with eaſe or propriety: an accompliſhment " abſolutely neceſſary to thoſe who wiſh to derive any advan- " tage from extenſive knowledge or profound erudition."

" Young People are naturally emulous of the company of " perſons reſpectable either for their Rank or Abilities, and if " they paſs their time with low or ignorant people it is for " want of being properly introduced and accuſtomed to mix in " the converſation of men of poliſhed manners and liberal ac- " compliſhments."————

—" The mode of Inſtruction here recommended, muſt neceſ- " ſarily in its Courſe remove theſe Defects, and the prejudices

" intailed on them; and promote the acquiſition of thoſe
" uſefull arts, on which the Succeſs of life ſo much depends."

" In the proſecution of it their Curioſity is excited and di-
" rected to proper objects, and a deſire for general knowledge
" the great foundation of excellence, implanted by making them
" acquainted with ſuch remarkable things in nature and art
" as are worthy of Attention. Inſtead of a ſlovenly or ungrace-
" full mode of ſpeaking, they are taught to deliver their ſenti-
" ments with propriety and preciſion according to times and
" circumſtances; conſequently their manner when improper
" is reformed inſenſibly, and without appearing an object of
" attention; their Minds are cultivated by frequent appeals
" to their underſtanding, and their judgements regulated by
" the conſtant habit of obſervation and reflection, exerciſed
" in a Minute enquiry into the Truth and propriety of ſuch
" thoughts as they are directed to in the courſe of their ſtudy.
" Thus being early qualified for the company of well bred and
" intelligent people they appear with becoming confidence and
" ſatisfaction on all occaſions."——

◆

PHILOSOPHICAL TRANSACTIONS.

XV. *New Experiments upon Gun-powder, with occaſional Obſervations and practical Inferences; to which are added, an Account of a new Method of determining the Velocities of all Kinds of Military Projectiles, and the Deſcription of a very accurate Eprouvette for Gun-powder. By* Benjamin Thompſon, *Eſq, F. R. S.*

Read March 29, 1781.

THESE experiments were undertaken principally with a view to determine the moſt advantageous ſituation for the vent in fire-arms, and to meaſure the velocities of bullets, and the recoil under various circumſtances. I had hopes alſo of being able to find out the velocity of the inflammation of gun-powder, and to meaſure its force more accurately than had hitherto been done. They were begun in the month of July in the year

1778, at Stoneland Lodge, a country ſeat of Lord GEORGE GERMAIN's, and I was aſſiſted by the reverend Mr. BALE, rector of Withyham, who lives in the neighbourhood.

The weather proved remarkably favourable for our experiments, being ſettled and ſerene, ſo that the courſe of them was never interrupted for a whole day by rain or by any accident. The mercury in the barometer ſtood in general pretty high, and the temperature of the atmoſphere was very equal, and moderately warm for the ſeaſon. In order that each experiment might, as nearly as poſſible, be under ſimilar circumſtances, they were all made between the hours of ten in the morning and five in the afternoon: and after each diſcharge the piece was wiped out with tow till all the inſide of the bore was perfectly clean, and as bright as if it had juſt come out of the hands of the maker; and great care was taken to allow ſuch a ſpace of time to elapſe between the firings, as might render the heat of the piece nearly the ſame in every experiment.

A deſcription of the apparatus.

The barrel principally uſed in theſe experiments was made by WOGDON, one of the moſt famous gunſmiths in London; and nothing can exceed the accuracy with which it is bored, or the fineneſs of the poliſh on the inſide. It is made of the very beſt iron, and, agreeably to Mr. ROBINS's advice, I took care to have it well fortified in every part, that there might be no danger of its burſting. Its weight and dimenſions may be ſeen in the table of the weight and dimenſions of the apparatus, p. 242.

Fig. 1. Repreſents a longitudinal ſection of a part of the barrel, with the apparatus firſt made uſe of for ſhifting the vent from one part of the chamber to another, or rather for moving

the bottom of the chamber further from, or bringing it nearer to, the vent, in order that the fire might be communicated to the powder in different parts of the charge.

a, *b*, repreſent the lower part of the barrel.

c, is the breech-pin, which is perforated with a hole four-tenths of an inch in diameter, the axis of which coincides with the axis of the bore.

Into this hole the ſcrew *h*, *n*, about four inches in length, is fitted; to the end of which, *n*, that paſſes up into the bore, is fixed a piſton *o*, *p*, which, by means of collars of oiled leather, is made to fit the bore of the piece very exactly. The end of the piſton *p*, neareſt the muzzle, is of braſs, and forms a moveable bottom to the bore, which by turning the ſcrew *h*, *n*, by means of the handle *m*, is brought nearer to, or removed further from, the fixed vent *v*, by which means the powder is lighted at any aſſignable diſtance from the bottom of the charge.

But the length of the bore being altered by moving the piſton, which occaſioned a ſmall inaccuracy, and ſome inconvenience attending the apparatus, it was laid aſide, and another repreſented by fig. 2. was ſubſtituted in the room of it.

a, *b*, is a ſection of part of the barrel as before, and *c* is the breech-pin, which being perforated with a ſmall hole through its center receives the ſcrew *f*, *g*, which is about two-tenths of an inch in diameter, and four inches long. This ſcrew being perforated with a very ſmall hole, ſerves to convey the fire into the chamber of the piece, and by ſcrewing it further up into the bore, or drawing it backwards, the fire is communicated to different parts of the charge.

But this method being found to be not intirely free from inaccuracies and inconveniencies, a third was ſubſtituted in the room of it, which was found to anſwer much better than either of the preceding.

The end of the bore was now firmly cloſed by a ſolid breech-pin *p*, fig. 3. and three vent holes *m*, *n*, and *o*, were made in the barrel; one of them, *m*, even with the bottom of the bore, and the other two at different diſtances from it. Any two of theſe vent holes, as *n* and *o* for inſtance, being cloſed up by ſolid ſcrews, a perforated ſcrew, or vent tube *v*, was ſcrewed into the third, which ſerved to contain the priming, and to convey the fire to the powder lodged in the bore of the piece.

Sometimes a longer vent-tube, repreſented by fig. 4. was made uſe of; which, paſſing through the powder in the chamber of the piece, communicated the fire immediately to that part of the charge that lay in the axis of the bore.

Another vent-tube alſo was uſed occaſionally, which differs in many reſpects from both thoſe that have been deſcribed. It is ſo conſtructed as to convey the fire to the charge; but, as ſoon as the powder in the chamber of the piece begins to kindle, and the elaſtic fluid to be generated, the vent is firmly cloſed by a valve, and no part of the generated fluid is permitted to eſcape. This I ſhall call the *valve-vent*, and it is repreſented by fig. 5. upon an enlarged ſcale, that the parts of it may appear more diſtinct.

a, *b*, is a longitudinal ſection of a ſmall portion of the ſolid ſide of the barrel.

c, *d*, is the vent-tube, which is in all reſpects like the ſhort vent-tube commonly made uſe of, except only that in this the end of the vent-hole (*e*) which goes into the chamber is enlarged in the form of the wide end of a trumpet or funnel.

To this enlarged aperture the valve, *v*, is accurately fitted, and by means of the ſmall ſtem or tail, *t*, which is fixed to the valve, and which paſſes up through the vent-hole, and is connected with the ſpring S, the valve is preſſed, or rather drawn into its place, and the vent is cloſed. The ſtem of the valve was at firſt made cylindrical; but, in order to make way for the priming to paſs down to the valve, one-half of its ſubſtance was taken away, as is repreſented in the figure.

When this vent is primed, the ſpace between the vent hole and the ſtem of the valve is filled with fine-grained powder, and the valve is gently opened by preſſing upon the end of the ſtem till one or more grains of powder lodge themſelves between the valve and the aperture; which preventing the valve from cloſing again, a ſmall opening is left for the paſſage of the flame into the chamber of the piece: therefore, when the priming is lighted, the fire paſſing down the vent, and entering the chamber, inflames the charge, and the ſmall grains of powder that were lodged between the valve and the aperture being deſtroyed by the flame in its paſſage through the vent, the valve immediately cloſes, and prevents the eſcape of any part of the elaſtic fluid generated by the inflammation of the powder in the chamber of the piece. The preſſure of this fluid upon the valve aſſiſts the action of the ſpring, by which means the valve is more expeditiouſly and more effectually cloſed.

The valve was very accurately fitted to the aperture by grinding them together with powdered emery, and afterwards poliſhing them one upon the other. And it is very certain, that no part of the elaſtic fluid made its eſcape by this vent; for, upon firing the piece, there was only a ſimple flaſh from the exploſion of the priming, and no ſtream of fire was to be ſeen iſſuing from the vent, as is always to be obſerved when a common vent is made uſe of, and in all other caſes where this fluid finds a paſſage.

In order that every part of the apparatus employed in theſe experiments might be as perfect as poſſible, all the more delicate parts of it were executed by Mr. FRASER, mathematical inſtrument-maker in Duke's Court, St. Martin's Lane, and, among the reſt, all the contrivances juſt deſcribed relative to the vent.

The velocities of the bullets were determined by means of a pendulum, according to the method invented by Mr. ROBINS.

The pendulum I made uſe of (fig. 6.) is compoſed of a circular plate of hammered iron (*a*), 13 inches in diameter, and 0,65 of an inch thick, to which is firmly faſtened a bar of iron (*b*, *c*) 56,5 inches in length, 2,6 inches broad, and half an inch in thickneſs, by which it is ſuſpended by means of two pivots (*d*, *e*) at the end of the bar (*c*), and at right angles to its length. Theſe pivots being very accurately finiſhed, and moving on poliſhed grooves, which were kept conſtantly oiled to leſſen the friction, the vibration of the pendulum was very free, as appeared by the great length of time its vibrations continued after it had been put in motion, and was left to itſelf. To the circular plate of the pendulum, targets of circular pieces of wood of different thickneſſes were fixed, which in the courſe of the experiments were often ſpoiled and replaced: and, in order to mark the weight and dimenſions of the pendulum in each experiment, the pendulums are numbered according to the different targets that were made uſe of; and the weight and dimenſions of each pendulum are ſet down in a table at the end of the deſcription of the apparatus.

The target of the pendulum N° 1. was made of a circular piece of elm-plank, 3½ inches thick, and equal in diameter to the iron plate of the pendulum to which it was fixed; but this target being too thin was very ſoon ruined.

The pendulum N° 2. was furniſhed with two targets, which were circular pieces of very tough oak-plank, near five inches thick, placed on oppoſite ſides of the plate of the pendulum, and firmly fixed to it by ſcrews, and to each other by iron ſtraps. When one of theſe targets was ruined, the pendulum was turned about, and the other was made uſe of. This pendulum laſted from experiment N°9. to experiment N°39. when it was ſo much ſhattered as to be rendered unfit for further ſervice.

The pendulum N° 3. was like the pendulum N° 2.; only, inſtead of oak, elm-plank near ſeven inches in thickneſs was made uſe of for the targets. This pendulum ſerved from experiment N° 40. to experiment N° 101. incluſively.

But finding that targets made of planks of the tougheſt wood were very ſoon ſhattered to pieces by the bullets, I compoſed the pendulum N° 4. in a different manner. Inſtead of circular pieces of plank, ſolid cylinders of elm-timber were made uſe of for the targets, ſo that the bullets now entered the wood in the direction of its fibres. Theſe cylinders are 13 inches in diameter, and about 5½ inches in length, hooped with iron at both their ends to prevent their ſplitting, and firmly faſtened to the plate of the pendulum, and to each other, by four iron ſtraps. This pendulum laſted till the experiments were finiſhed. It is ſtill in being, and appears to be very little the worſe for the ſervice it has undergone.

Fig. 7. ſhews the two ends of the pendulum upon a large ſcale, together with the hooks or grooves by which it was ſuſpended.

a, *b*, is the bar of the pendulum, which is ſeen broken off, as there is not room to ſhew the whole of its length.

c, *d*, are the pivots by which it was ſuſpended.

e, is the circular plate of the pendulum, to which

f, *g*, two circular targets, are faſtened by ſcrews, and by means of the iron ſtraps, 1, 2, 3, 4, which are nailed to the edges of the targets.

h, *k*, are the hooks which ſerved inſtead of grooves to receive the pivots, *c*, *d*, of the pendulum.

The hooks were firmly fixed to the horizontal beam R. S. which ſupported the whole apparatus by means of three ſcrews *m*, *n*, *o*, which paſſed through three holes in the plate that connects the two hooks. When the hooks were faſtened to the beam, the middle ſcrew, *n*, was firſt put into its place, and the pendulum was allowed to ſettle itſelf in a poſition truly perpendicular, after which the grooves were immoveably fixed by means of the ſcrews *m*, *o*.

The chord of the arc, through which the pendulum aſcended in each experiment, was meaſured by a ribbon, according to the method invented and deſcribed by Mr. ROBINS.

The recoil was meaſured in the following manner. The barrel was ſuſpended in an horizontal poſition (and nearly in a line with the center of the target) by two ſmall pendulous rods, 64 inches in length, and 25,6 inches aſunder; which being parallel to each other, and moving freely upon poliſhed pivots about the axes of their ſuſpenſion, and upon two pair of trunnions that were fixed to the barrel, formed, together with the barrel, a compound pendulum; and from the lengths of the vibrations of this pendulum, the velocity with which the barrel began to recoil, or rather its greateſt velocity, was determined.

But in order that the velocity of the recoil might not be too great, ſo as to endanger the apparatus when large charges were made uſe of, it was found neceſſary to load the barrel with an additional weight of more than 40 lbs. of iron.

This additional weight of iron, which I ſhall call the *gun carriage*, as it was ſo conſtructed as to ſerve as a carriage to the barrel, is compoſed of a bar of hammered iron 28 inches in length, 2,6 inches broad, and half an inch in thickneſs, which is bent in the middle of its length in ſuch a manner, that its two flat ſides or ends are parallel to each other, and diſtant aſunder two inches. In the middle of this bar where it is bent is a hole in the form of an oblong ſquare, which, receiving the end of the breech-pin, ſupports the lower end or breech of the barrel. The other end of the barrel is ſupported and confined in the following manner. A ring or hoop of iron, near half an inch thick, and two inches in diameter, is placed in a vertical poſition between the parallel ſides of the bar, and near its two ends, and firmly fixed to them by ſcrews. The barrel paſſing through the middle of this ring is ſupported upon the ends of three ſcrews, which paſſing through the ring in different parts of its circumference all point towards its center.

The carriage, together with the barrel, was ſuſpended by the pendulous rods by means of two pair of poliſhed trunnions that are fixed to the outſide of the carriage. They are placed in an horizontal line perpendicular to, and paſſing through, the axis of the bore.

Fig. 8. repreſents the barrel fixed to the carriage.

a, *b*, *c*, is the bar of iron which forms the carriage ſeen edge-ways.

2, 2, 4, 4, are the trunnions by which it was ſuſpended.

d, *e*, is the barrel in its proper place.

p, is the breech-pin, which paſſing through a hole in the middle of the bar, *a*, *b*, *c*, ſupports the end, *e*, of the barrel; and

n, is the ring that ſupports the end, *d*, of the barrel.

Fig. 9. repreſents a perpendicular ſection through the line 2, 2, fig. 8, and in a line perpendicular to the length of the barrel.

This figure is deſigned to ſhew the manner in which the muzzle of the piece was ſupported and confined in the ring *n*, fig. 8.

a, *c*, are the two ends of the bar that are ſeen cut off.

n, is the ring, and

p, *p*, are the ſcrews by which it is faſtened to the two parallel ſides of the bar, the ends of which form the trunnions 2, 2, fig. 8.

d, is a tranſverſe ſection of the barrel, and

r, *s*, *t*, are the three ſcrews by which the barrel is ſupported and confined in the center of the ring.

Fig. 10. is the ſame as fig. 9. but upon a larger ſcale.

Fig. 11. repreſents the two ends of one of the pendulous rods by which the barrel was ſuſpended; and fig. 13, ſhews the ſame ſeen ſideways.

a, *b*, is the rod which is ſeen broken off.

c, *d*, are the pivots by which it was ſuſpended by a pair of hooks or grooves that were faſtened to an horizontal beam, in the ſame manner as the pendulum for meaſuring the velocities of the bullets was ſuſpended.

e, *f*, are the hooks which receive the trunnions that are fixed to the carriage.

The dimenſions of every part of this apparatus may be ſeen in the table, p. 242.

The chord of the arc through which the barrel aſcended in its recoil was meaſured by a ribbon, and the lengths of thoſe chords, expreſſed in inches and decimal parts of an inch, are ſet down in the tables. The method of computing the velocity of the recoil from the chord of the arc through which the barrel aſcended, is too well known to require an explanation; and it is alſo well known, that the velocities are to each other as the chords of thoſe arcs. The lengths of thoſe chords, therefore, as they are ſet down in the tables, are, in all caſes, as the velocities of the recoil.

The powder made uſe of in theſe experiments was of the beſt kind, ſuch as is uſed in proving great guns at Woolwich. A cartridge, containing 12 lbs. of this powder, was given to me by the late General DESAGULIERS of the Royal Artillery, and Inſpector of Braſs and Iron Ordnance; who alſo, in the politeſt manner, offered me every other aſſiſtance in his power towards completing the experiments I had projected, or in making any others I ſhould propoſe that might be uſeful in the proſecution of my inquiries.

This powder was immediately taken out of the cartridge, and put into glaſs bottles, which were previouſly made very clean and dry; and in theſe it was kept carefully ſealed up till it was opened for uſe. When it was wanted for the experiments, it was weighed out in a very exact balance, with ſo much attention, that there could not poſſibly be an error in any inſtance greater than one quarter part of a grain. The bottles were never opened but in fine weather, and in a room that was free from damp, and no more charges of powder than were neceſſary for the experiments of the day were weighed out at a time. Each charge was carefully put up in a cartridge of very fine paper, and theſe filled cartridges were kept in a turned wooden box, that was varniſhed on the inſide as well as the outſide, to prevent its imbibing moiſture from the air.

The paper of which theſe cartridges were made was ſo fine and thin, that 1280 ſheets of it made no more than an inch in

thickneſs,

thickneſs, and a cartridge capable of containing half an ounce of powder weighed but three quarters of a grain.

The cartridges were formed upon a wooden cylinder, and accurately fitted to the bore of the piece, and the edges of the paper were faſtened together with paſte made of flour and water.

When a cartridge was filled, the powder was gently ſhaken together, and its mouth was tied up and ſecured with a piece of fine thread; and when it was made uſe of it was put intire into the piece, and gently puſhed down into its place with the ram-rod, and afterwards it was pricked with a priming-wire thruſt through the vent, and the piece was primed; ſo that no part of the powder of the charge was loſt in the act of loading, as is always the caſe when the powder is put looſe into the barrel: nor was any part of it expended in priming; but the whole quantity was ſafely lodged in the bottom of the bore or chamber of the piece, and the bullet was put down immediately upon it, without any wadding either between the cartridge and the bullet, or over the bullet.

The bullets were all caſt in the ſame mould, and conſequently could not vary in their weights above two or three grains at moſt, eſpecially as I took care to bring the mould to a proper temperature as to heat before I began caſting; and when leather was put about them, or other bullets than thoſe of lead were made uſe of, the weight was determined very exactly before they were put into the piece.

The diameter of the bullet was determined by meaſurement and alſo by computation from its weight, and the ſpecific gravity of the metal of which it was formed; and both theſe methods gave the ſame dimenſions very nearly.

The apparatus was put up for making the experiments in a coach-houſe, which was found very convenient for the purpoſe, as the joiſts upon which the floor over head was laid afforded a firm and commodious ſupport for ſuſpending the pendulum and the barrel, and the walls and roofs of the building ſerved to ſcreen the apparatus, which otherwiſe might have been diſcompoſed by the wind, and injured by the rain and dews. A pair of very large doors, which formed the whole of one end of the room, were kept conſtantly open during the time the experiments were making, in order to preſerve the purity of the air within the houſe, which otherwiſe would have been much injured by the ſmoke of the gun-powder; and that, in all probability, would have had a conſiderable effect in leſſening the force of the powder, and vitiating the experiments. In order ſtill further to guard againſt this evil, the barrel was placed as near as poſſible to the door, and the pendulum was hung up at the bottom of the room.

Fig. 12. repreſents the apparatus as it was put up for making the experiments.

a, *b*, is the barrel with its carriage, ſuſpended by the pendulous rods *c*, *d*, and

R, is the ribbon which ſerved to meaſure the aſcending arc of its recoil.

P, is the pendulum, and

r, the ribbon that meaſured the arc of its vibration.

The diſtance from the mouth of the piece to the pendulum was juſt 12 feet.

A table

A table ſhewing the weights and dimenſions of all the principal parts of the apparatus.

Of the barrel.

	Inches.
Length	44,7
Length of the bore from the muzzle to the breech-pin	43,45
Diameter of the bore	0,78
Thickneſs of metal at the lower vent	0,36
Thickneſs of metal at the muzzle	0,1

Weight of the barrel, together with the ſolid breech-pin, and the vent-ſcrews and vent-tube, 6 lbs. 6 oz.

Of the gun carriage.

Length	28,4
Diſtance between the two pair of trunnions	25,6
Diameter of each trunnion	0,25

Weight 40 lbs. 14 oz.

Of the rods by which the carriage was ſuſpended.

Length from the axis of ſuſpenſion, or center of the pivots, to the center of the trunnions of the gun carriage, 64 inches.

Weight of each rod, 1 lb. 4 oz.

Total weight of the barrel and its carriage, together with the allowance that was made for the weight of the rods by which it was ſuſpended, 48 lbs.

N. B. This was its weight from experiment N° 3. to experiment N° 123. incluſive.

Of the bullet.

Diameter 0,75 of an inch.
Weight in lead 580 grains.

Of the pendulum.

	Inches.
Total length of the pendulum from the axis of ſuſpenſion to the bottom of the circular plate	69,5
Diameter of the circular plate to which the targets were faſtened	13,-
Diſtance between the ſhoulders of the pivots	3,8
Diameter of the pivots	,27

Weight of the iron part of the pendulum 47 lb. 4 oz.

Of the pendulum with the targets fixed to it, as it was prepared for making the experiments, and numbered.

	Total length to the ribbon.	Diſtance from the axis of ſuſpenſion.		Total weight of iron and wood.	
		To the center of gravity.	To the center of oſcillation.		
	Inches.	Inches.	Inches.	lbs.	oz.
Pendulum N° 1	69,25	50,25	58,45	57	0
——— N° 2	69,5	54,4	59,15	82	4
——— N° 3	——	55,62	60,23	100	12
——— N° 4	——	54,6	59,18	88	4

N. B. The meaſure is Engliſh feet and inches, and the weight is avoirdupois.

Having now gone through the deſcription of all the principal parts of the apparatus, I ſhall proceed to give an account of the experiments. And as it may be ſatisfactory to the Society to ſee the method of conducting theſe enquiries, as well as the reſult of them, I ſhall firſt give a table of the experiments in the exact order in which they were made, together with my original remarks; I ſhall then make ſuch general obſervations as may occur: and afterwards I ſhall ſelect, combine, and compare them, in the manner which beſt anſwers the different purpoſes to which I ſhall apply them.

General

General table of the experiments.

In the two firſt experiments the barrel was fixed to a carriage (that has not been deſcribed) which, together with the barrel and rods by which it was ſuſpended, weighed only 23½.

Length of the bore of the piece 43,5 inches.

Weight of the bullet 580 grains.

The pendulum, N° 1.

Order of the experiments.	The charge of powder.		Vent from the bottom of the charge.	Chord of the aſcending arc of the pendulum.	The bullet ſtruck the target below the axis of the pendulum.	Chord of the arc of the recoil.	Velocity of the bullet.	Remarks.
	Weight.	Height.						
	Grs.	In.	In.	Inches.	Inches.	In.	Ft. in Sec.	
N° 1	208	1,8	0,	13,2	64,5	33,5	1267	Firſt day.
2	. .	. .	,5	14,5	. . .	36,5	1399	

This gun carriage being found to be too light, the other, deſcribed, and repreſented fig. 8. was ſubſtituted in the room of it.

Order of the experiments.	The charge of powder.		Vent from the bottom of the charge.	Chord of the aſcending arc of the pendulum.	The bullet ſtruck the target below the axis of the pendulum.	Chord of the arc of the recoil.	Velocity of the bullet.	Remarks.
	Weight.	Height.						
	Grs.	In.	In.	Inches.	Inches.	Inches.	Ft. in ſec.	
N° 3	208	1,8	0	12,6	65,	17,8	1213	Second day.
4	. .	. .	,5	——	——	18,5	—	The pendulum gave way.
5	. .	. .	0	——	——	38,68	—	4 bullets were fired at once.
6	. .	. .	,5	——	——	38,48	—	Ditto.
7	. .	. .	0	——	——	6,1	—	Without any bullet.
8	416	3,6	. .	——	——	16,5	—	Ditto.
9	208	1,8	0	8,5	65,	17,69	1281	Pen. N° 2; very fair; 3d day.
10	104	,9	. .	5,2	65,25	10,18	782	
11	310	2,7	0	9,6	64,6	24,69	1459	The powder was lighted by the long vent-tube (fig. 4.).
12	. .	. .	1,22	10,1	65,	24,95	1527	
13	. .	. .	2,65	11,85	64,75	24,9	1801	
14	. .	. .	. .	10,9	65,25	. . .	1646	
15	330	2,9	2,65	10,9	61,5	26,2	1748	
16	. .	. .	. .	13,25	63,5	. . .	2060	
17	330	2,7	2,65	. . .	. . .	12,7	. .	The barrel very much heated.
18	. .	2,9	0	10,4	63,5	26,3	1619	
19	. .	. .	. .	. . .	63,	26,4	1633	
20	165	1,45	0	6,8	62,2	14,73	1084	
21	. .	. .	. .	6,85	. . .	14,2	1093	
22	. .	. .	1,32	6,7	. . .	14,8	1071	
23	. .	. .	. .	6 3	60,6	14,58	1035	The ſhort vent-tube (*v*, fig. 3.) was made uſe of.
24	. .	. .	. .	7,5	61,5	14,68	1142	

In order to determine how much of the force of the powder was loſt by windage and by the vent, oiled leather was faſtened round the bullet, ſo that it now accurately fitted the bore of the piece; and in the five experiments, from N° 35. to N° 39. incluſive, the valve-vent was made uſe of.

Weight of the bullet, together with the leather in which it was enveloped, 603 grains.

Order of the experiments.	The charge of powder.		Vent from the bottom of the charge.	Chord of the aſcending arc of the pendulum.	The bullet ſtruck the target below the axis of the pendulum.	Chord of the arc of the recoil.	Velocity of the bullet.	Remarks.
	Weight.	Height.						
N°	Grs.	In.	In.	Inches.	Inches.	Inches.	Ft. in ſec.	
25	165	1,45	0	6,8	65,	14,95	1004	Fourth day.
26	. .	. .	. .	7,8	. . .	15,6	1153	
27	. .	. .	. .	8,05	. . .	16,15	1192	
28	330	2,9	. .	10,2	63,	26,	1559	
29	. .	. .	2,6	. . .	64,	28,1	1536	
30	165	3,2	. .	5,9	62,4	13,2	914	
31	. .	1,45	1,3	6,65	62,6	15,15	1027	

Finding that the blaſt of the powder always reached as far as the pendulum, when large charges were made uſe of, and ſuſpecting that this circumſtance, together with the impulſe of the unfired grains, might in a great meaſure occaſion the apparent irregularity in the velocities of the bullets; to remedy theſe inconveniences, a large ſheet of paper of a moderate thickneſs was ſtretched upon a ſquare frame of wood, and interpoſed as a ſcreen before the pendulum at the diſtance of two feet from the ſurface of the target.

Two reaſons conſpired to induce me to prefer this method of preventing the impulſe of the flame upon the pendulum to the obvious one of removing the pendulum further from the mouth of the piece; the firſt was, that I was unwilling to increaſe the diſtance between the barrel and the pendulum, leſt the reſiſtance of the air might affect the velocities of the bullets; and the ſecond, which I confeſs did not operate leſs ſtrongly than the firſt, was, that the length of the houſe did not admit of a greater diſtance, and I was unwilling to expoſe any part of the apparatus in the open air.

But the ſcreen was found to anſwer perfectly well the purpoſe for which it was deſigned, and it was continued during the remainder of the experiments, the paper being replaced every third or fourth experiment.

The experiments continued.

Order of the experiments.	The charge of powder.		Vent from the bottom of the charge.	Chord of the aſcending arc of the pendulum.	Bullet ſtruck the target below the axis of the pendulum.	Chord of the arc of the recoil.	Velocity of the bullet.	Remarks.
	Weight.	Height.						
N°	Grs.	In.	In.	Inches.	Inches.	Inches.	Ft. in ſec.	
32	165	1,45	0	5,45	63,	15,45	839	Not leathered; weight of the bullet and wad 603 grs. In exp. N° 32. no leſs than 40 large grs. of unfited powder were driven through the ſcreen.
33	. .	. .	. .	. . .	. . .	12,65	839	
34	. .	. .	. .	7,9	. . .	15,45	1217	In theſe 6 experiments the bullets were leathered, and the powder was lighted by the valve-vent.
35	. .	. .	. .	7,	60,25	15,25	1129	
36	. .	. .	. .	7,4	62,	16,3	1161	
37	. .	. .	1,3	8,	61,	17,9	1277	
38	290	2,6	2,6	9,	58,6	23,5	1497	
39	. .	. .	. .	. . .	. . .	24.8	. .	The pend. N° 2. ruined.

The bullets were now put naked into the piece, and the powder was lighted by the ſhort vent-tube (*v*, fig. 3) and ſome little improvement was made in the ſteel edges between which the ribbons paſſed that ſerved to meaſure the aſcending arcs of the pendulum and of the recoil, by which means the friction was leſſened, and the ribbon was prevented from twiſting or entangling itſelf as it was drawn out.

Apparatus.

The barrel with its carriage as before.
The pendulum, N° 3. and
Leaden bullets, weighing 580 grains each.

Order of the experiments.	The charge of powder.		Vent from the bottom of the charge.	Chord of the aſcending arc of the pendulum.	Bullet ſtruck the target below the axis of the pendulum.	Chord of the arc of the recoil.	Velocity of the bullet.	Remarks.
	Weight.	Height.						
N°	Grs.	In.	In.	Inches.	Inches.	Inches.	Ft. in ſec.	
40	218	1,9	0	6,45	64,6	18,	1236	5th day; medium velocity in theſe experiments and N° 47. 1225.
41	. .	. .	. .	6,31	65,3	17,71	1197	
42	. .	. .	. .	6,45	65,	17,91	1230	
43	. .	. .	1,3	6,5	64,6	18,3	1248	Medium velocity 1276
44	. .	. .	. .	6,75	64,5	18,35	1299	
45	. .	. .	. .	6,6	64,9	. . .	1265	
46	. .	. .	. .	6,4	61,6	. . .	1293	
47	. .	. .	0	6,3	62,	18,1	1266	
48	290	2,6	0	7,2	63,5	22,58	1414	Medium velocity 1427.
49	. .	. .	. .	7,4	. . .	22,92	1455	
50	. .	. .	. .	7,3	64,6	22,38	1412	
51	290	2,6	1,3	7,4	63,	23,21	1476	Medium velocity 1493.
52	. .	. .	. .	7,6	64,	23,76	1520	
53	. .	. .	. .	7,25	61,	23,6	1483	
54	. .	. .	2,6	7,5	62,3	. . .	1502	Medium velocity 1460.
55	. .	. .	. .	7,4	64,	23,26	1450	
56	. .	. .	. .	7,1	62,2	. . .	1433	
57	. .	. .	. .	7,4	64,	23 56	1454	
58	. .	. .	. .	1,31		11,12	—	In theſe 4 experiments the piece was fired with powder alone, and the ſcreen was taken away from before the pendulum.
59	. .	. .	. .	1,2		11,62	—	
60	. .	. .	0	1,16		9,62	—	
61	. .	. .	1,3	0,6		11,33	—	

Order of the experiments.	The charge of powder.		Vent from the bottom of the charge.	Chord of the aſcending arc of the pendulum.	Bullet ſtruck the target below the axis of the pendulum.	Chord of the arc of the recoil.	Velocity of the bullet.	Remarks.
	Weight.	Height.						
	Grs.	In.	In.	Inches.	Inches.	Inches.	Ft. in ſec.	
N°								
62	330	2,9	1,3	8,	63,	26,4	1599	6th day; medium velocity 1625.
63	. .	. .	. .	8,5	65,	. . .	1652	
64	. .	. .	2,6	7,2	59,5	25,3	1562	Medium velocity 1528.
65	. .	. .	. .	7,7	65,	. . .	1495	
66	. .	. .	0	8,4	. . .	26,35	1633	Medium velocity 1594.
67	. .	. .	. .	8,	. . .	25,8	1556	
68	218	1,9	0	6,82	64,	19,56	1349	The powder was rammed very hard.
69	. .	. .	. .	6,6	64,6	18,2	1294	Ditto much harder.
70	. .	. .	. .	6,85	. . .	19,12	1345	Ditto as hard as in N° 68.
71	. .	. .	1,3	5,5	. . .	16,33	1080	Ditto, ditto.
72	. .	. .	0	—	—	8,72	—	Government powder, no bullet.
73	. .	. .	. .	—	—	8,44	—	Beſt double battle powder.
74	. .	. .	1,3	—	—	8,47	—	Government powder.
75	. .	. .	. .	—	—	9,3	—	Double battle powder.

The following experiments N° 78, 79, 80, and 81. were made in hopes of being able to diſcover a method of adding to the force of gun-powder. Twenty grains of the ſubſtances mentioned in the remarks upon each experiment were intimately mixed with the powder of the charge. In the experiment N° 82. a large wad of tow, well ſoaked in etherial ſpirit of turpentine, was put into the piece immediately upon the bullet: and in the experiment N° 83. a wad, ſoaked in alkohol, was put into the piece in like manner.

Order of the experiments.	The charge of powder.		Vent from the bottom of the charge.	Chord of the aſcending arc of the pendulum.	Bullet ſtruck the target below the axis of the pendulum.	Chord of the arc of the recoil.	Velocity of the bullet.	Remarks.
	Weight.	Height.						
N°	Grs.	In.	In.	Inches.	Inches.	Inches.	Ft. in ſec.	
76	145	1,3	0	5,3	65,	13,25	1037	7th day; medium velocity 1040.
77	..	..	..	...	64,6	13,25	1044	
78	..	..	..	3,2	...	8 92		20 grs beſt alkaline ſalt of tartar.
79	..	..	..	4,35	...	11,68		20 grs. æthiops mineral.
80	..	..	..	3,3	63,6	9,83		20 grs. ſal ammon.
81	..	..	..	4,2	63,4	11,45		20 grs. fine braſs duſt.
82	..	..	..	——	——	15,25	—	The ſcrews which held the hooks by which the pendulum was ſuſpended gave way, and the pendulum came down.
83	..	..	..	——	——	14,35	—	

In the nine following experiments, *viz.* from N° 84. to N° 92. incluſive, the valve-vent was made uſe of, and the bullets were made to fit the bore of the piece very exactly by means of oiled leather, which was ſo firmly faſtened about them that in each experiment it entered the target with the bullet.

The bullet made uſe of in experiment N° 85. was of wood.

Thoſe uſed in the experiments N° 86. and N° 87. were formed in the following manner; a ſmall bullet was caſt of plaiſter of Paris, which being thoroughly dried, and well heated at the fire, was fixed in the center of the mould that ſerved for caſting all the leaden bullets made uſe of in theſe experiments; and melted lead being poured into this mould, the cavity that ſurrounded the ſmall plaiſter bullet was intirely filled up, and a bullet was produced, which to the eye had every appearance of ſolidity, but was as much lighter than a ſolid leaden bullet of the ſame diameter as the plaiſter bullet was lighter than a leaden bullet of the ſame ſize.

In the experiments N° 88. and N° 89. ſolid leaden bullets were made uſe of. In the experiment N° 90. two bullets were diſcharged at once; in the experiment N° 91. three; and in the experiment N° 92. four were uſed.

In each of theſe experiments a freſh ſheet of paper was made uſe of as a ſcreen to the pendulum, in order that the velocities of the bullets might be meaſured more accurately; and alſo, that the quantity of unfired powder might be eſtimated with greater preciſion.

Order of the experiments.	The charge of powder.		Vent from the bottom of the charge.	Weight of the bullet.	Chord of the aſcending arc of the pendulum.	Bullet ſtruck the target below the axis of the pendulum.	Chord of the arc of recoil.	Velocity of the bullet.	**Remarks.**
	Weight.	Height.							
N°	Grs.	In.	In.	Grs.	Inches.	Inches.	Inches.	Ft. in ſec.	
84	145	1,3	0	—	—	—	4,5	—	8th day; in each of theſe 4 experimepes from 50 to 70 granulæ or particles of unfired powder were driven through the ſcreen.
85	..	..	..	90	1,33	62,2	7,16	1763	
86	..	..	..	251	2,82	63,2	9,62	1317	
87	..	..	..	354	3,32	61,2	11,3	1136	
88	..	..	..	600	6,5	65,4	15,22	1229	Very few unfired grains of powder ſtruck the ſcreen.
89	..	..	..	603	6,3	64,6	15,13	1229	
90	..	..	..	1184	10,12	65,	21,92	978	There were no marks of any unfired powder having reached the ſcreen.
91	..	..	..	1754	13,65	63,4	27,18	916	
92	..	..	..	2352	16,55	63,3	32,25	833	

In the ſeven following experiments the piece was fired with powder only.

Order of the experiments.	The charge of powder.		Vent from the bottom of the charge.	Chord of the aſcending arc of the pendulum.	Chord of the arc of the recoil.	Remarks.
	Weight	Height.				
N°	Grs.	In.	In.	Inches.	Inches.	
93	145	1,3	0	——	4,3	
94	165	1,45	..	——	5,5	
95	..	..	..	——	5,6	
96	290	2,6	..	——	11,70	
97	437½	3,9	..	1,68	17,5	The ſcreen was taken away.
98	...	...	..	6,7	15,88	The whole ſurface of the target was beſpattered with unfired grains of powder.
99	..	..	..	——	17,9	The pendulum was not obſerved.

In the following experiments N° 100. and N° 101. the bullets were not put down into the bore, but were ſupported by three wires, which being faſtened to the end of the barrel projected beyond it, and confined the bullet in ſuch a ſituation that its center was in a line with the axis of the bore, and its hinder part was one-twentieth of an inch without or beyond the mouth of the piece.

In experiment N° 102. the bullet was juſt ſtuck into the barrel in ſuch a manner that near one-half of it was without the bore.

Order of the experiments.	The charge of powder.		Vent from the bottom of the charge.	Chord of the aſcending arc of the pendulum.	Bullet ſtruck the target below the axis of the pendulum.	Chord of the arc of the recoil.	Velocity of the bullet.	Remarks.
	Weight.	Height.						
	Grs.	In.	In.	Inches.	Inches.	Inches.	Ft. in ſec.	
Nº								In each of theſe experim near $\frac{1}{10}$th part of the ſubſtance of the bullet was melted and blown away by the impulſe of the flame.
100	165	1,45	0	,65	60,5	4,9	138	
101	. .	. .	. .	,43	uncertain.	4,8	92	
102	. .	. .	. .	,80	63,	5,6	180	

All that part of the bullet which lay towards the bore of the piece appeared to be quite flat from the loſs of ſubſtance it had ſuſtained; and its ſurface was full of ſmall indents, which probably were occaſioned by the unfired grains of powder that impinged againſt it.

The following experiments were made with the pendulum N° 4. The reſt of the apparatus as before.

Order of the experiments	The charge of powder.		Vent from the bottom of the charge.	Chord of the aſcending arc of the pendulum.	Bullet ſtruck the target below the axis of the pendulum.	Chord of the arc of the recoil.	Velocity of the bullet.	Remarks.
	Weight.	Height.						
	In.	In.	In.	Inches.	Inches.	Inches.	Ft. in ſec.	
N°								9th day.
103	104	,9	0	4,51	65,	10,6	732	About 40 grains of powder were driven through the ſcreen.
104	145	1,3	. .	5.4	. . .	12,92	877	About 40 unfired grs. of powder. Medium velocity 894.
105	. .	. .	. .	5,6	. . .	13,28	910	40 unfired grains.
106	. .	1,14	. .	6,18	65,8	14,3	990	Double proof battle powder; no unfired grains.
107	218	1,8	. .	8,48	65,	19,68	1380	Ditto, ditto.
108	290	2,6	. .	9,45	65 6	23,9	1526	Government powder; bullet leathered; weight 602 grains.
109	. .	. .	. .	8,73	65,2	22,8	1419	Bullet naked; very few unfired grains. Medium velocity 1444.
110	. .	. .	. .	9.3	65,6	23,4	1460	
111	. .	. .	. .	. . .	. . .	. . .	1462	
112	. .	. .	. .	8,85	65,5	22,94	1436	
113	. .	. .	2,6	8,65	64,	23,7	1438	Medium velocity 1413.
114	. .	. .	. .	8,5	63,6	24,1	1423	
115	. .	. .	. .	8,4	65,	23,8	1378	
116	. .	2,28	. .	9,15	64,	24,6	1525	Double proof battle powder.
117	437½	3,9	. .	10,56	64,9	33,	1738	Gov. pow. No unfired grains thro' the ſcreen. Medium velocity 1764.
118	. .	. .	. .	11,	64,5	33,3	1824	
119	. .	. .	. .	10,5	65,	33,6	1729	
120	. .	. .	2,6	10,35	. . .	32,5	1706	Medium velocity 1751.
121	. .	. .	. .	10,65	. . .	33,2	1757	
122	. .	. .	. .	10,6	63,6	32,9	1789	
123	. .	. .	0	——	——	17,9	—	Without any bullet.

Of the method made uſe of for computing the velocities of the bullets.

As the method of computing the velocity of a bullet from the arc of the vibration of a pendulum into which it is fired is ſo well known, I ſhall not enlarge upon it in this place, but ſhall juſt give the theorems that have been propoſed by different authors, and ſhall refer thoſe who wiſh to ſee more on the ſubject to Mr. ROBINS's New Principles of Gunnery; to Profeſſor EULER's Obſervations upon Mr. ROBINS's Book; and, laſtly, to Dr. HUTTON's Paper on the initial Velocities of Cannon Balls, which is publiſhed in the Tranſactions of the Society for the year 1778.

If a denote the length from the axis of the pendulum to the ribbon which meaſures the chord of the arc of its vibration;

g, the diſtance of the center of gravity below the axis;

f, the diſtance of the center of oſcillation;

b, the diſtance of the point ſtruck by the bullet;

c, the chord of the aſcending arc of the pendulum;

P, the weight of the pendulum;

b, the weight of the bullet, and

v, the original velocity of the bullet;

$v = \frac{c}{a} \times \overline{\frac{Pg}{bb} + \frac{b}{f}} \times \frac{f}{\sqrt{2b}}$, is a theorem for finding the velocity upon Mr. ROBINS's principles.

*$v = \frac{c}{a} \times \overline{\frac{Pg}{bb} + \frac{f+b}{2f}} \times \sqrt{\frac{f}{2}}$, is the theorem propoſed by Profeſſor EULER, who has corrected a ſmall error in Mr. ROBINS's method; and

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* Put the rational part $\frac{c}{a} \times \overline{\frac{Pg}{bb} + \frac{f+b}{2f}} = n$, and expreſs f in the thouſandth parts of a Rhynland foot; then the velocity with which the ball ſtrikes the pendulum will be $= \frac{n}{4}\sqrt{\frac{f}{2}}$ Rhynland feet in a ſecond.

$v =$

$v = 5,672\,cg\sqrt{f} \times \frac{P+b}{bha}$ is Dr. HUTTON's theorem, which is ſufficiently accurate, and far more ſimple and expeditious than either of the preceding. It is to be remembered, that g, h, and c, may be expreſſed in any meaſure; but f muſt be Engliſh feet, and v will be the velocity of the bullet in Engliſh feet in a ſecond.

The velocities of the bullets in moſt of the foregoing experiments were firſt computed by EULER's method, as I had not then ſeen Dr. HUTTON's paper; but in going over the calculations a ſecond time, I made uſe of Dr. HUTTON's theorem. Both theſe methods gave the ſame velocity very nearly, but the Doctor's method is by much the eaſieſt in practice.

In theſe computations care was taken to make a proper allowance for the bullets that were lodged in the pendulum, and alſo for the velocity loſt by the bullet in paſſing through the ſcreen.

The corrections neceſſary on account of the bullets lodged in the pendulum were made in the following manner.

b was continually added to the value of P,

$\frac{b-g}{P} \times b$ . . . to the value of g, and

$\frac{f-h}{P} \times b$ . . . to the value of f.

Of the ſpaces occupied by the different charges of powder.

The heights of the charges of powder, or the lengths of the ſpaces which they occupied in the bore, were determined by meaſurement; and in order that this might be done with greater accuracy, inches and tenths of inches were marked upon the ram-rod, and the charge was gently forced down till it occupied the ſame ſpace in each experiment.

The following table ſhews the heights of the charges as they were determined by meaſurement, and alſo their heights computed from the diameter of the bore of the piece, and the ſpecific gravity of the powder that was made uſe of.

N. B. By an experiment I ſhall give an account of hereafter, I found the ſpecific gravity of this powder ſhaken well together to be to that of rain water as 0,937 is to 1,000.

Weight of the powder.	Height of the charge.	
	Meaſured.	Computed.
Grs.	Inches.	Inches.
104	,9	0,8957
145	1,3	1,2490
165	1,45	1,4211
208	1,8	1,7914
218	1,9	1,8775
290	2,6	2,4980
310	2,7	2,6700
330	2,9	2,8422
416	3,6	3,5828
437½	3,9	3·7680

In the experiment N° 30. the powder was put into a cartridge ſo much ſmaller than the bore of the piece, that the charge, inſtead of occupying 1,45 inches, extended 3,2 inches. By this diſpoſition of the powder, its action upon the bullet appears to have been very much diminiſhed.

Of the effect that the heat which pieces acquire in firing produces upon the force of powder.

It is very probable, that the exceſs of the velocity of the bullet in the ſecond experiment over that of the firſt was occaſioned more by the heat the barrel had acquired in the firſt experiment than by the poſition of the vent, or any other circumſtance; for I have ſince found, upon repeated trials, that the force of any given charge of powder is conſiderably greater when it is fired in a piece that has been previouſly heated by firing, or by any other means, than when the piece has not been heated. Every body that is acquainted with artillery knows, that the recoil of great guns is much more violent after the ſecond or third diſcharge than it is at firſt; and on ſhip-board, where it is neceſſary to attend to the recoil of the guns, in order to prevent very dangerous accidents that might be occaſioned by it, the conſtant practice has been in our navy, and, I believe, on board the ſhips of all other nations, to leſſen the quantity of powder after the firſt four or five rounds: our 32 pounders, for inſtance, are commonly fired with 14 lbs. of powder at the beginning of an action, but the charge is very ſoon reduced to 11 lbs. and afterwards to 9 lbs. and the filled cartridges are prepared accordingly.

By the recoil it ſhould ſeem, that the powder exerted a greater force alſo in the fourth experiment, being the ſecond upon the ſecond day, than it did upon the third, or the firſt upon that day; but the pendulum giving way, it was not poſſible to compare the velocities of the bullets in the manner we did in the two experiments mentioned above.

This augmentation of the force of powder, when it is fired in a piece that is warm, may be accounted for in the following manner. There is no ſubſtance we are acquainted with that does not require to be heated before it will burn; even gunpowder is not inflammable when it is cold. Great numbers of ſparks or red-hot particles from the flint and ſteel are frequently ſeen to light upon the priming of a muſket, without ſetting fire to the powder, and grains of powder may be made to paſs through the flame of a candle without taking the fire; and what is ſtill more extraordinary, if large grains of powder are let fall from the height of two or three feet upon a red-hot plate of iron, laid at an angle of about 45° with the plane of the horizon, they will rebound intire without being burnt, or in the leaſt altered, by the experiment. In all theſe caſes the fire is too feeble, or the duration of its action is not ſufficiently long to heat the powder to that degree which is neceſſary in order to its being rendered inflammable.

Now as gun-powder, as well as all other bodies, acquires heat by degrees, and as ſome ſpace of time is taken up in this as well as in all other operations, it follows, that powder, which has been warmed by being put into a piece made hot by repeated firing, is much nearer that ſtate in which it will burn, or, I may ſay, is more inflammable than powder which is cold; conſequently, more of it will take fire in a given ſhort ſpace of time, and its action upon the bullet and upon the gun will of courſe be greater.

The heat of the piece will alſo ſerve to dry the air in the bore, and to clear the inſide of the gun of the moiſture that collects there when it has not been fired for ſome time, and theſe circumſtances doubtleſs contribute ſomething to the quickneſs of the inflammation of the powder, and conſequently to its force.

As it takes a longer time to heat a large body than a ſmall one, it follows, that meal-powder is more inflammable than that which is grained; and the ſmaller the particles are, the quicker they will take fire. The ſailors bruiſe the priming after they have put it to their guns, as they find it very difficult, without this precaution, to fire them off with a match: and if thoſe who are fond of ſporting would make uſe of a ſimilar artifice, and prime their pieces with meal-powder, they would miſs fire leſs often, the ſprings of the lock might be made more tender, and its ſize conſiderably reduced without any riſque, and the violence of the blow of the flint and ſteel in ſtriking fire being leſſened, the piece might be fired with greater preciſion.

Concluding from the reſult of the four experiments mentioned above, as well as from the reaſons juſt cited, that the temperature of the piece has a conſiderable effect upon the force of the powder, I afterwards took care to bring the barrel to a proper degree of heat, by firing it once or oftener with powder each time I recommenced the experiments after the piece had been left to cool.

Of the manner in which pieces acquire heat in firing.

I was much ſurpriſed upon taking hold of the barrel immediately after the experiment N° 17. when it was fired with 330 grains of powder without any bullet, to find it ſo very hot that I could ſcarcely bear it in my hand; evidently much hotter than I had ever obſerved it before, notwithſtanding the ſame charge of powder had been made uſe of in the two preceding experiments, and in both theſe experiments the piece was loaded with a bullet, which one would naturally imagine, by confining the flame, and prolonging the time of its action, would heat the barrel much more than when it was fired with powder alone.

I was convinced that I could not be miſtaken in the fact, for it had been my conſtant practice to take hold of the piece to wipe it out as ſoon as an experiment was finiſhed, and I never before had found any inconvenience from the heat in holding it. But in order to put the matter beyond all doubt, after letting the barrel cool down to the proper temperature, I repeated the experiment twice with the ſame charge of powder and a bullet; and in both theſe trials (experiments N° 18. and N° 19.) the heat of the piece was evidently much leſs than what it was in the experiment above mentioned (N° 17.).

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I now regretted exceedingly the loſs of a ſmall pocket thermometer, which I had provided on purpoſe to meaſure the heat of the barrel, but it was accidentally broken by a fall the day before I began my experiments; and being ſo far from London, I had it not in my power to procure another: I was therefore obliged to content myſelf with determining the heat of the piece as well as I could by the touch.

Being much ſtruck with this accidental diſcovery of the great degree of heat that pieces acquire when they are fired with powder without any bullet, and being deſirous of finding out whether it is a circumſtance that obtains univerſally, I was very attentive to the heat of the barrel after each of the ſucceeding experiments; and I conſtantly found the heat ſenſibly greater when the piece was fired with powder only, than when the ſame charge was made to impel one or more bullets.

Though the reſult of theſe experiments was totally unexpected, and even contrary to what I ſhould have foretold if I had been aſked an opinion upon the ſubject previous to making them; yet, after mature conſideration, I am now convinced, that it is what ought to happen, and that it may be accounted for very well upon principles that are clearly admiſſible.

It is certain, that a very ſmall part only of the heat that a piece of ordnance acquires in being fired is communicated to it by the flame of the powder; for the time of its action is ſo ſhort (not being, perhaps, in general longer than about $\frac{1}{200}$th or $\frac{1}{150}$th part of a ſecond) that if its heat, inſtead of being 4 times, as Mr. ROBINS ſuppoſes, was 400 times hotter than red-hot iron, it could not ſenſibly warm ſo large a body of metal as goes to form one of our large pieces of cannon. And beſides, if the heat of the flame was ſufficiently intenſe to produce ſo great an effect in ſo ſhort a time, it would certainly be ſufficient to burn up all inflammable bodies that it came near, and to melt the ſhot that it ſurrounded and impelled, eſpecially when they were ſmall, and were compoſed of lead or any other ſoft metal; but, on the contrary, we frequently ſee the fineſt paper come out of the mouth of a piece uninflamed, after it has ſuſtained the action of the fire through the whole length of the bore, and the ſmalleſt lead ſhot is diſcharged without being melted.

But it may be objected here, that bullets are always found to be very hot if they are taken up immediately after they come out of a gun; and that this circumſtance is a proof of the intenſity of the heat of the flame of powder, and of its great power of communicating heat to the denſeſt bodies. But to this I anſwer, I have always obſerved the ſame thing of bullets diſcharged from wind-guns and croſs-bows, eſpecially when they have impinged againſt any hard body, and are much flattened; and bullets from muſkets are always found to be hotter in proportion to the hardneſs of the body againſt which they are fired. If a muſket ball is fired into any very ſoft body, as (for inſtance) into water, it will not be found to be ſenſibly warmed; but if it is fired againſt a thick plate of iron, or any other body that it cannot penetrate, the bullet will be demoliſhed by the blow, and the pieces of it that are diſperſed about will be found to be in a ſtate very little ſhort of fuſion, as I have often found by experience. It is not by the flame therefore that bullets are heated, but by percuſſion. They may, indeed, receive ſome ſmall degree of warmth from the flame, and ſtill more perhaps by friction againſt the ſides of the bore, but it is in ſtriking againſt hard bodies, and from the reſiſtance they meet with in penetrating thoſe that are ſofter, that they acquire by far

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the greater part of the heat we find in them as ſoon as they come to be at reſt, after having been diſcharged from a gun.

There is another circumſtance that may poſſibly be brought as an objection to this opinion, and that is the running of the metal in braſs guns upon repeatedly firing them, by which means the vent is often ſo far enlarged as to render the piece intirely uſeleſs. But this, I think, proves nothing but that braſs is very eaſily corroded, and deſtroyed by the flame of gun-powder; for it cannot be ſuppoſed, that in theſe caſes the metal is ever fairly melted. The vent of a muſket is very ſoon enlarged by firing, and after a long courſe of ſervice it is found neceſſary to ſtop it up with a ſolid ſcrew, through the center of which a new vent is made of the proper dimenſions. This operation is called buſhing, or rather bouching the piece; but in all the better kind of fowling-pieces the vent is lined, or bouched, with gold, and they are found to ſtand fire for any length of time without receiving the leaſt injury. But every body knows that gold will run with a leſs heat than is required to melt iron: but gold is not corroded either by the ſpirit of nitre, or the acid ſpirit that is generated from ſulphur, whereas iron is very eaſily deſtroyed by either; and that I take to be the only reaſon why a vent that is lined with gold is ſo much more durable than one that is made in iron. But it ſeems, that iron is more durable than braſs; and perhaps ſteel, or ſome other cheap metal, may be found that will ſupply the place of gold, and by that means the great expence that attends bouching pieces with that precious metal may be ſpared, and this improvement may be introduced into common uſe.

This leads us to a very eaſy and effectual remedy for that defect ſo long complained of in all kinds of braſs ordnance, *the running of the vent*; for if theſe pieces were bouched with iron, there is no doubt but they would ſtand fire as well as iron guns; and if ſteel, or any other metal, either ſimple or compounded, ſhould upon trial be found to anſwer for that purpoſe better than iron, it might be uſed inſtead of it; and even if gold was made uſe of for lining the vent, I imagine it might be done in ſuch a manner as that the expence would not be very conſiderable, at the ſame time that the thickneſs of the gold ſhould be ſufficient to withſtand the force of the flame for a very great length of time.

But to return to the heat acquired by guns in firing. It being pretty evident that it is not all communicated by the flame, there is but one other cauſe to which it can be attributed, and that is the motion and friction of the internal parts of the metal among themſelves, occaſioned by the ſudden and violent effort of the powder upon the inſide of the bore, and to this cauſe I imagine the heat is principally if not almoſt intirely owing. It is well known, that a very great degree of heat may be generated in any hard and denſe body in a ſhort ſpace of time by friction, and in a ſtill ſhorter time by colliſion. "For if two denſe hard elaſtic bodies be ſtruck againſt
" each other with great force and velocity, all the parts of
" ſuch bodies will every moment be cloſely compreſſed; and
" being rigid will re-act with equal force. Hence a quick and
" powerful contraction and expanſion will ariſe in every part,
" reſembling that ſwift kind of vibrations obſerved in ſtretched
" ſtrings; how great theſe vibrations are may be learnt from
" the inſtance of a bell, when ſtruck with a ſingle blow, by
" which the whole bulk, however vaſt, will for a long time
" expand, and contract itſelf in infinite ellipſes. And when
" the attrition above deſcribed is produced, with what force and
" velocity are all the particles of the rubbed body compreſſed,

" ſhaken, and looſened to their very intimate ſubſtance*?" And in proportion to the ſwiftneſs of this vibration, and the violence of the attrition and friction, will be the heat that is produced.

A piece of iron that would ſuſtain the preſſure of any weight, however large, without being warmed, may be made quite hot by the blow of a hammer; and even ſoft and un-elaſtic bodies may be warmed by percuſſion, provided the velocity with which their parts are made to give way to the blow is ſufficiently rapid. If a leaden bullet is laid upon an anvil, or any other hard body, and in that ſituation it is ſtruck with a ſmart blow of a hammer, it will be found to be much heated; but the ſame bullet in the ſame ſituation may be much more flattened by preſſure, or by the ſtroke of a very heavy body moving with a ſmall velocity, without being ſenſibly warmed.

To generate heat therefore the action of the powder upon the inſide of the piece muſt not only be ſufficient to ſtrain the metal, and produce a motion in its parts, but this effect muſt be extremely rapid; and the heat will be much augmented, if the exertion of the force and the duration of its action are momentaneous; for in that caſe, the fibres of the metal (if I may uſe the expreſſion) that are violently ſtretched, will return with their full force and velocity, and the ſwift vibratory motion and attrition before deſcribed will be produced.

The heat generated in a piece by firing is therefore as the force by which the particles of the metal are ſtrained and compreſſed, the ſuddenneſs with which this force is exerted, and the ſhortneſs of the time of its action; that is to ſay, as the ſtrength of the powder and the quantity of the charge, the quickneſs of its inflammation, and the velocity with which the generated fluid makes its eſcape.

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* Vide SHAW's tranſlation of BOERHAAVE's Chemiſtry, vol. I. p. 249.

Now the effort of any given charge of powder upon the gun is very nearly the ſame, whether it be fired with a bullet or without; but the velocity with which the generated elaſtic fluid makes its eſcape, is much greater when the powder is fired alone, than when it is made to impel one or more bullets; the heat ought therefore to be greater in the former caſe than in the latter, as I found by experiment.

But to make this matter ſtill plainer, we will ſuppoſe any given quantity of powder to be confined in a ſpace that is juſt capable of containing it, and that in this ſituation it is by any means ſet on fire. Let us ſuppoſe this ſpace to be the chamber of a piece of ordnance of any kind, and that a bullet, or any other ſolid body, is ſo firmly fixed in the bore immediately upon the charge, that the whole effort of the powder ſhall not be able to remove it. As the powder goes on to be inflamed, and the elaſtic fluid is generated, the preſſure upon the inſide of the chamber will be increaſed, till at length all the powder being burnt, the ſtrain upon the metal will be at its greateſt height, and in this ſituation things will remain, the coheſion or elaſticity of the particles of metal counterbalancing the preſſure of the fluid.

Under theſe circumſtances very little heat would be generated; for the continued effort of the elaſtic fluid would approach to the nature of the preſſure of a weight; and that concuſſion, vibration, and friction, among the particles of the metal, which in the colliſion of elaſtic bodies is the cauſe of the heat that is produced, would ſcarcely take effect.

But inſtead of being firmly fixed in its place, let the bullet now be moveable, but let it give way with great difficulty, and by ſlow degrees. In this caſe, the elaſtic fluid will be generated as before, and will exert its whole force upon the chamber of the piece; but as the bullet gives way to the preſſure, and moves on in the bore, the fluid will expand itſelf and grow weaker, and the particles of the metal will gradually return to their former ſituations; but the velocity with which the metal reſtores itſelf being but ſmall, the vibration that remains in the metal, after the elaſtic fluid has made its eſcape, will be very languid, as will be the heat that is generated by it.

But if, inſtead of giving way with ſo much difficulty, the bullet is much lighter, ſo as to afford but little reſiſtance to the elaſtic fluid in making its eſcape, or if the powder is fired without any bullet at all; then, there being little or nothing to oppoſe the flame in its paſſage through the bore, it will expand itſelf with an amazing velocity, and its action upon the gun will ceaſe almoſt in an inſtant, the ſtrained metal will reſtore itſelf with a very rapid motion, and a ſharp vibration will enſue, by which the piece will be much heated.

Of the effect of ramming the powder in the chamber of the piece.

The charge, conſiſting of 218 grains of powder, being put gently into the bore of the piece in a cartridge of very fine paper, without being rammed, the velocity of the bullets at a mean of the 40th, 41ſt, 42d, and 47th experiments, was at the rate of 1225 feet in a ſecond; but in the 68th, 69th, and 70th experiments, when the ſame quantity of powder was rammed down with five or ſix hard ſtrokes of the ram-rod, the mean velocity was 1329 feet in a ſecond. Now the total force or preſſure exerted by the charge upon the bullet is as the ſquare of its velocity, and $\overline{1329}^2$ is to $\overline{1225}^2$ as 1,1776 is to 1; or nearly as 6 is to 5; and in that proportion was the force of the given charge of powder increaſed by being rammed.

In the 71ſt experiment the powder was alſo rammed, but the vent, inſtead of being at the bottom of the bore, was at 1,3, and the velocity of the bullet was very conſiderably diminiſhed, being only at the rate of 1080 feet in a ſecond, inſtead of 1276 feet in a ſecond, which was the mean velocity with this charge, and with the vent in this ſituation when the powder was rammed. See the experiments N^os. 43, 44, 45, and 46.

When, inſtead of ramming the powder, or preſſing it gently together in the bore, it is put into a ſpace larger than it is capable of filling, the force of the charge is thereby very ſenſibly leſſened, as Mr. ROBINS and others have found by repeated trials. In my 30th experiment the charge, conſiſting of no more than 165 grains of powder, was made to occupy 3,2 inches of the bore inſtead of 1,45 inches, which ſpace it juſt filled when it was gently puſhed into its place without being rammed; the conſequence was, the velocity of the bullet, inſtead of being 1100 feet in a ſecond or upwards, was only at the rate of 914 feet in a ſecond, and the recoil was leſſened in proportion.

And from hence we may draw this practical inference, that the powder, with which a piece of ordnance or a fire-arm is charged, ought always to be preſſed together in the bore; and if it is rammed to a certain degree, the velocity of the bullet will be ſtill farther increaſed. It is well known, that the recoil of a muſket is greater when its charge is rammed than when it is not; and there cannot be a ſtronger proof that ramming increaſes the force of the powder.

Of the relation of the velocities of bullets to the charges of powder by which they are impelled.

It appears by all the experiments that have hitherto been made upon the initial velocities of bullets, that when the weights and dimenſions of the bullets are the ſame, and they are diſcharged from the ſame piece by different quantities of powder, the velocities are in the ſub-duplicate ratio of the weights of the charges very nearly.

The following table will ſhew how accurately this law obtained in the foregoing experiments.

Charges.	Velocities		Difference.	N° of exp.
	Computed.	Actual.		
437½	1764	1764	——	3
330	1533	1594	+ 61	2
310	1486	1459	− 27	1
290	1436	1436	0	7
218	1232	1225	− 7	4
208	1216	1256	+ 40	3
165	1083	1087	+ 4	2
145	1018	1040	+ 22	2
104	860	757	− 103	2

The computed velocities, as they are ſet down in this table, were determined from the ratio of the ſquare root of 437½ (the weight in grains of the largeſt charge of powder) to the mean velocity of the bullet with that charge and the vent at 0; *viz.* 1764 feet in a ſecond, and the ſquare root of the other charges expreſſed in grains. And the *actual* velocities are means of all experiments that were made under ſimilar circumſtances.

The fourth column ſhews the difference of the computed and actual velocities, or the number of feet in a ſecond by which the actual velocity exceeds or falls ſhort of the computed: and in the fifth column is ſet down the number of experiments with each charge, from the mean of which the actual velocity was determined.

The agreement of the computed and actual velocities will appear more ſtriking, if we take the ſum and difference of thoſe velocities with all the charges except the firſt: thus,

Sum of the velocities, – 1764.

Computed.	Actual.	Difference.	N° of exp.
9864	9854	– 10	23

So that it appears, that the difference, or the actual velocity, was ſmaller than the computed by $\frac{1}{985}$ part only at a mean of 23 experiments.

But as by far the greater number of the experiments were made with the following charges, *viz.* 290, 218, 208, 165, and 145 grains of powder, let us take the ſum and difference of the computed and actual velocities of thoſe charges: thus,

Sum of the velocities

Computed.	Actual.	Difference.	N° of exp.
5985	6044	+ 59	18

Here the agreement of the theory with the experiments is ſo very remarkable, that we muſt ſuppoſe it was in ſome meaſure accidental; for the difference of the velocities in repeating the ſame experiment is in general much greater than the difference of the computed and actual velocities in this inſtance; but, I think, we may fairly conclude, from the reſult of all theſe trials, that the velocities of like *muſket* bullets, when they are diſcharged from the ſame piece by different quantities of the ſame kind of powder, are very nearly in the ſub-duplicate ratio of the weights of the charges. Whether this law will hold good when applied to cannon balls, and bomb ſhells of large dimenſions, I dare not at preſent take upon me to decide; but, for ſeveral reaſons that might be mentioned, I am rather of opinion, that it will not; at leaſt not with that degree of accuracy which obtained in theſe experiments.

Of the effect of placing the vent in different parts of the charge.

There have been two opinions with reſpect to the manner in which gun-powder takes fire. Mr. ROBINS ſuppoſes that the progreſs of its inflammation is ſo extremely rapid, "that all "the powder of the charge is fired and converted into an elaſtic "fluid, before the bullet is ſenſibly moved from its place;" while others have been of opinion, that the progreſs of the inflammation is much ſlower, and that the charge is ſeldom or never completely inflamed before the bullet is out of the gun.

The large quantities of powder that are frequently blown out of fire arms un-inflamed, ſeem to favour the opinion of the advocates for the gradual firing; but Mr. ROBINS endeavours to account for that circumſtance upon different principles, and ſupports his opinion by ſhewing that every increaſe of the charge within the limits of practice produces a proportional increaſe of the velocity of the bullet, and that when the powder is confined by a great additional weight, by firing two or more bullets at a time inſtead of one, the velocity is not ſenſibly greater than it ought to be according to his theory.

If this were a queſtion merely ſpeculative, it might not be worth while to ſpend much time in the diſcuſſion of it; but as it is a matter upon the knowledge of which depends the determination of many important points reſpecting artillery, and from which many uſeful improvements may be derived, too much pains cannot be taken to come at the truth. Till the manner in which powder takes fire, and the velocity with which the inflammation is propagated, are known, nothing can with certainty be determined with reſpect to the beſt form for the chambers of pieces of ordnance, or the moſt advantageous ſituation for the vent; nor can the force of powder, or the ſtrength that is required in different parts of the gun, be aſcertained with any degree of preciſion.

As it would be eaſy to determine the beſt ſituation for the vent from the velocity of the inflammation of powder being known, ſo on the other hand I had hopes of being able to come at that velocity by determining the effect of placing the vent in different parts of the charge; for which purpoſe the following experiments were made.

A table of experiments, ſhewing the effect of placing the vent in different parts of the charge.

Weight of the charge of powder.	Space occupied by the powder.	Vent from the bottom of the bore.	Velocity of the bullet at a medium.	Recoil meaſured upon the ribbon at a medium.	Number of experiments.
Grains.	Inches.	Inches.	Ft. in a ſec.	Inches.	
165	1,45	0	1087	14,465	2
. . .	. . .	1,32	1082	14,31	3
218	1,9	0	1225	17,93	4
. . .	. . .	1,3	1276	18,34	4
290	2,6	0	1427	22,626	3
. . .	. . .	1,3	1493	23,34	3
. . .	. . .	2,6	1460	23,286	4
. . .	. . .	0	1444	23,135	4
. . .	. . .	2,6	1413	24,5	3
310	2,7	0	——	24,69	1
. . .	. . .	1,32	——	24,95	1
. . .	. . .	2,65	——	24,9	1
330	2,9	0	1594	26,075	2
. . .	. . .	1,3	1625	26,4	2
. . .	. . .	2,6	1525	25,3	2
437½	3,9	0	1764	33,3	3
. . .	. . .	2,6	1751	32,866	3

By the foregoing experiments it appears, firſt, that the difference in the force of any given charge of powder which ariſes from the particular ſituation of the vent is extremely ſmall.

With 165 grains of powder, and the vent at 0, the velocity of the bullet at a mean of two experiments (*viz.* the 20th and 21ſt) was 1087 feet in a ſecond; and with the ſame charge, and the vent at 1,32 inches, the velocity at a mean of the 22d, 23d, and 24th experiments, was 1082 feet in a ſecond;

the difference, equal five feet in a ſecond, is leſs than what occurred in a repetition of the ſame experiment.

With 218 grains of powder, and the vent at o, the velocity at a mean in the 40th, 41ſt, 42d, and 47th experiments, was at the rate of 1225 feet in a ſecond; and with the ſame charge, and the vent at 1,3, the velocity was 1276 feet in a ſecond at a mean of four experiments, *viz.* the 43d, 44th, 45th, and 46th.

In the firſt ſet of experiments, with 290 grains of powder, the velocities were,

	Vent at o.	Vent at 1,3.	Vent at 2,6.
	1414	1476	1502
	1455	1520	1450
	1412	1483	1433
			1454
	3)4281	3)4479	4)5839
Means	1427	1493	1460

See the experiments from N° 48. to N° 57. incluſive.

In the ſecond ſet the velocities were,

	Vent at o.	Vent at 2,6.
	1419	1438
	1460	1423
	1462	1378
	1436	
	4)5777	3)4239
Means	1444	1413

See the experiments from N° 109. to N° 115. incluſive.

And taking the means of all the velocities in both ſets in each poſition of the vent it will be,

	Vent at 0.	Vent at 1,3.	Vent at 2,6.
Mean velocity	1436	1493	1437

The mean recoils in theſe experiments were,

Vent at 0.	Vent at 1,3.	Vent at 2,6.
22,88	23,34	23,61

In the experiments with 310 grains of powder the velocities of the bullets were not determined with ſufficient accuracy to be depended on; but the recoils, which were meaſured with great nicety, were as follows, *viz.*

Vent at 0.	Vent at 1,3.	Vent at 2,6
24,69	24,95	24,9

With 330 grains of powder the mean velocities and recoils were,

	Vent at 0.	Vent at 1,3.	Vent at 2,6.
Velocities	1594	1625	1525
Recoils	26,075	26,4	25,3

In the experiments with 437½ grains (an ounce avoirdupois) of powder the velocities and recoils were,

	Vent at 0.		Vent at 2,6.	
	Velocity.	Recoil.	Velocity.	Recoil.
	1738	33,	1707	32,5
	1824	33,3	1757	33,2
	1728	33,6	1789	32,9
	3)5291	3)99,9	3)5253	3)98,6
Means	1764	33,3	1751	32,866

Secondly, From the reſult of all theſe experiments it appears, that the effect of placing the vent in different poſitions with reſpect to the bottom of the chamber is different, in different charges; thus, with 165 grains of powder the velocity of the bullet was rather diminiſhed by removing the vent from 0, or the bottom of the bore to 1,32; but with 218 grains of powder the velocity was a little increaſed, as was alſo the recoil. With 290 grains of powder the velocity was greateſt when the powder was lighted at the vent 1,3 which was near the middle of the charge, and rather greater when it was lighted at the top, or immediately behind the bullet, than when it was lighted at the bottom. And by the recoil it would ſeem, that the velocities of the bullets varied nearly in the ſame manner when the charge conſiſted of 310 grains of powder.

With 330 grains of powder, both the velocity and the recoil were greater when the powder was lighted at the middle of the charge, than when it was lighted at the bottom; but they were leaſt of all when it was lighted near the top. And when *an ounce* of powder was made uſe of for the charge, its force was greateſt when it was lighted at the bottom. But the difference in the force exerted by the powder which aroſe from the particular poſition of the vent was in all caſes ſo inconſiderable (being, as I have before obſerved, leſs than what frequently occurred in repeating the ſame experiment) that no concluſion can be drawn from the experiments, except only this, that any given charge of powder exerts nearly the ſame force, whatever is the poſition of the vent.

And hence the following practical inference naturally occurs, *viz.* that in the formation of fire-arms no regard need be had to any ſuppoſed advantages that gun-ſmiths and others have hitherto imagined were to be derived from particular ſituations for the vent, ſuch as diminiſhing the recoil, increaſing the force of the charge, &c.; but the vent may be indifferently in any part of the chamber where it will beſt anſwer upon other accounts: and there is little doubt but the ſame thing will hold good in great guns, and all kinds of heavy artillery.

Almoſt every workman who is at all curious in fire-arms has a particular fancy with regard to the beſt form for the bottom of the chamber, and the proper poſition of the vent. They in general agree, that the vent ſhould be as low or far back as poſſible, in order, as they pretend, to leſſen the recoil; but no two of them make it exactly in the ſame manner. Some make the bottom of the chamber flat, and bring the vent out even with the end of the breech-pin. Others make the vent ſlanting through the breech-pin, in ſuch a manner as to enter the bore juſt in its axis. Others again make the bottom of the chamber conical; and there are thoſe who make a little cylindric cavity in the breech-pin, of about two-tenths of an inch in diameter, and near half an inch in length, coinciding with the axis of the bore, and bring out the vent even with the bottom of this little cavity.

The objection to the firſt method is, the vent is apt to be ſtopped up by the foul matter that adheres to the piece after firing, and which is apt to accumulate, eſpecially in damp weather. The ſame inconvenience in a ſtill greater degree attends the other methods, with the addition of another, ariſing from the increaſed length of the vent; for the vent being longer it is not only more liable to be obſtructed, but it takes a longer time for the flame to paſs through it into the chamber, in conſequence of which the piece is ſlower in going off, or, as ſportſmen term it, is apt to hang fire.

The form I would recommend for the bottom of the bore is that of a hemiſphere; and the vent ſhould be brought out directly through the ſide of the barrel, in a line perpendicular to its axis, and pointing to the center of the hemiſpheric concavity of the chamber.

In this caſe the vent would be the ſhorteſt poſſible; it would be the leaſt liable to be obſtructed, and the piece would be more eaſily cleaned, than if the bottom of the bore was of any other form. All theſe advantages, and ſeveral others not leſs important, would be gained by making the bottom of the bore and vent of great guns in the ſame manner.

A new method of determining the velocities of bullets.

From the equality of *action* and *re-action* it appears, that the *momentum* of a gun muſt be preciſely equal to the momentum of its charge; or that the weight of the gun, multiplied into the velocity of its recoil, is juſt equal to the weight of the bullet and of the powder (or the elaſtic fluid that is generated from it) multiplied into their reſpective velocities: for every particle of matter, whether ſolid or fluid, that iſſues out of the mouth of a piece, muſt be impelled by the action of ſome power, which power muſt *re-act* with equal force againſt the bottom of the bore.

Even the fine inviſible elaſtic fluid that is generated from the powder in its inflammation cannot put itſelf in motion without re-acting againſt the gun at the ſame time. Thus we ſee pieces, when they are fired with powder alone, recoil as well as when their charges are made to impel a weight of ſhot, though the recoil is not in the ſame degree in both caſes.

It is eaſy to determine the velocity of the reccil in any given caſe, by ſuſpending the gun in an horizontal poſition by two pendulous rods, and meaſuring the arc of its aſcent, by means of a ribbon according to the method already deſcribed, and this will give the momentum of the gun, its weight being known, and conſequently the momentum of its charge. But in order to determine the velocity of the bullet from the recoil, it will be neceſſary to find out how much the weight and velocity of the elaſtic fluid contributes to it.

That part of the recoil which ariſes from the expanſion of this fluid is always very nearly the ſame, whether the powder is fired alone, or whether the charge is made to impel one or more bullets, as I have found by a great variety of experiments.

If therefore a gun, ſuſpended according to the method preſcribed, is fired with any given charge of powder, but without any bullet or wad, and the recoil is obſerved, and if the ſame piece is afterwards fired with the ſame quantity of powder, and a bullet of a known weight, the exceſs of the velocity of the recoil in the latter caſe, over that in the former, will be proportional to the velocity of the bullet; for the difference of theſe velocities, multiplied into the weight of the gun, will be equal to the weight of the bullet multiplied into its velocity.

Thus if W is put equal to the weight of the gun,

U = the velocity of its recoil, when it is fired with any given charge of powder, without any bullet,

V = the velocity of the recoil, when the ſame charge is made to impel a bullet,

B = the weight of the bullet, and

v = its velocity,

It will be $v = \frac{\overline{V-U} \times W}{B}$.

Let us ſee how this method of determining the velocities of bullets will anſwer in practice.

In the 94th experiment the recoil, with 165 grains of powder, without a bullet, was 5,5 inches, and in the 95th experiment, with the ſame charge, the recoil was 5,6 inches. The mean is 5,55 inches; and the length of the rods by which the barrel was ſuſpended being 64 inches, the velocity of the recoil (= U) anſwering to 5,55 inches meaſured upon the ribbon, is that of 1,1358 feet in a ſecond.

In five experiments, with the ſame charge of powder, and a bullet weighing 580 grains, the recoil was as follows, *viz.*

The 20th experiment		14,73 inches
21ſt	-	14,2
22d	-	14,8
23d	-	14,58
24th	-	14,68
		5)73, (= 14,6 inches at a mean.

And the velocity of the recoil (= V) anſwering to the length is that of 2,9880 feet in a ſecond: conſequently V - U, or 2,9880 - 1,1358 is equal to 1,8522 feet in a ſecond.

But as the velocities of recoil are known to be as the chords of the arcs through which the barrel aſcends, it is not neceſſary in order to determine the velocity of the bullet to compute the velocities V and U; but the quantity $\overline{V-U}$, or the difference of the velocities of the recoil when the given charge is fired with and without a bullet, may be computed from the value of the difference of the chords, by one operation. Thus the velocity anſwering to the chord 9,05 is that of 1,8522 feet in a ſecond, which is juſt equal to $\overline{V-U}$, as was before found.

The weight of the barrel, together with its carriage, was 47¼ pounds, to which three quarters of a pound is to be added on account of the weight of the rods by which it was ſuſpended, which makes W = 48 pounds, or 336,000 grains, and the weight of the bullet was 580 grains. B is therefore to W as 580 is to 336,000, that is, as 1 is to 579,31 very nearly; and v ($= \frac{\overline{V-U}\times W}{B}$ is equal to $\overline{V-U} \times 579{,}31$.

The value of $\overline{V-U}$ anſwering to the experiments before mentioned was found to be 1,8522; conſequently the velocity of the bullets ($= v$) was $1{,}8522 \times 579{,}31 = 1073$ feet in a ſecond, which is extremely near 1083 feet in a ſecond, the mean of the velocities, as they were determined by the pendulum.

But the computation for determining the velocity of a bullet upon theſe principles may be rendered ſtill more ſimple and eaſy in practice; for the velocities of the recoil being as the chords meaſured upon the ribbon, if

c is put equal to the chord of the recoil expreſſed in Engliſh inches, when the piece is fired with powder only, and

C = the chord when a bullet is diſcharged by the ſame charge,

then $C - c$ will be as $V - U$; and conſequently as $\frac{\overline{V-U}\times W}{B}$, which meaſures the velocity of the bullet, the ratio of W to B remaining the ſame.

If therefore we ſuppoſe a caſe in which $C - c$ is equal to one inch, and the velocity of the bullet is computed from that chord, the velocity in any other caſe, wherein $C - c$ is greater or leſs than one inch, will be found by multiplying the difference of the chords C and c by the velocity that anſwers to a difference of one inch.

The length of the parallel rods by which the barrel was ſuſpended being 64 inches, the velocity of the recoilanſwering to C − *c* = 1 inch meaſured upon the ribbon is 0,204655 parts of a foot in a ſecond; and this is alſo, in this caſe the value of V − U; the velocity of the bullet is therefore v = 0,204655 × 579,31 = 118,35 feet in a ſecond.

Conſequently the velocity of the bullet expreſſed in feet *per* ſecond may in all caſes be found by multiplying the difference of the chords C, and *c*, by 118,35, the weight of the barrel, the length of the rods by which it is ſuſpended, and the weight of the bullet remaining the ſame, and this whatever the charge of powder may be that is made uſe of, and however it may differ in ſtrength or goodneſs.

According to this rule the velocities of the bullets in the following experiments have been computed from the recoil; and by comparing them with the velocities ſhewn by the pendulum, we ſhall be enabled to judge of the accuracy of this new method of determining the velocities of bullets.

In the 76th and 77th experiments, when the piece was fired with 145 grains of powder and a bullet, the recoil was 13,25 and 13,15, or 13,2 at a mean; and with the ſame charge of powder, without a bullet, the recoil was 4,5 and 4,3, or 4,4 at a mean (ſee the 84th and 93d experiments). C − *c* is therefore 13,2 − 4,4 = 8,8 inches, and the velocity of the bullets = 8,8 × 118,35 = 1045 feet in a ſecond. The mean of the velocities as they were determined by the pendulum is that of 1040 feet in a ſecond. In the 104th and 105th experiments the recoil was 12.92 and 13,28, and the velocity computed from the mean of thoſe chords is 1030 feet in a ſecond; but the velocity ſhewn by the pendulum was no more than about 900 feet in a ſecond. As the recoil was ſo nearly equal to what it was in the 76th and 77th experiments before mentiond, when the velocities ſhewn by the recoil and by the pendulum were almoſt exactly the ſame, I am inclined to believe, that there muſt have been ſome miſtake in determining the velocities by the pendulum in theſe laſt experiments, and that the velocity ſhewn by the recoil is moſt to be depended on.

With 290 grains, or half the weight of the bullet in powder, in the 48th, 49th, and 50th experiments, the recoil was 22,58, 22,92, and 22,38; and the recoil, with the ſame charge of powder, without a bullet, at a mean of the 60th and 99th experiments, was 10,66. The mean of the velocities of the bullets, computed from the recoil, is therefore 1416 feet in a ſecond, and the velocity ſhewn by the pendulum was 1427 feet in a ſecond: the difference is not conſiderable. The mean of the velocities in the 109th, 110th, 111th, and 112th experiments is by the recoil 1464, and by the pendulum 1444 feet in a ſecond.

With 330 grains of powder the velocities of the bullets appear to have been as follows, *viz.*

	Vent at 0.	Vent at 1,3.	Vent at 2,6.
By the recoil	1543	1620	1610
By the pendulum	1594	1625	1528

See the 62d, 63d, 64th, 65th, 66th, 67th, and 17th experiments.

The uniformity of the recoil was in all caſes very remarkable. Thus, in the firſt ſet of experiments with 290 grains of powder (from the 48th to the 57th experiment incluſive), the recoil was,

	Vent at o.	Vent at 1,3.	Vent at 2,6.
	22,58	23,21	23,06
	22,92	23,76	23,26
	22,38	23,06	23,26
			23,56
	3)67,88	3)70,03	4)93,14
Means =	22,626	23,343 and	23,285

If now we take a mean of the 60th and 99th experiments, and call the recoil, without a bullet, 10,66 as before, the velocities will turn out,

	Vent at o.	Vent at 1,3.	Vent at 2,6.
By the recoil - -	1416	1501	1494
And by the pendulum they were	1427	1493	1494
The difference is only .	− 11	+ 8	+ 34

The recoil was equally regular in the 117th and five ſucceeding experiments, when the charge was no leſs than 437½ grs. = 1 ounce avoirdupois in powder; and the velocities of the bullets determined from the recoil are very nearly the ſame as they were ſhewn by the pendulum. Thus, in the 117th, 118th, and 119th experiments the mean recoil was 33,3; and in the 120th, 121ſt and 122d experiments it was 32,866. And if the recoil without a bullet is called 17,9, as it was determined by the 123d experiment, which was made immediately after the experiments before mentioned, then will the velocities be,

	Vent at o.	Vent at 2,6
By the recoil - -	1822	1771
And by the pendulum they were	1764	1751

The difference is only - +58 and +20 feet in a ſecond, which is leſs than what frequently occurs in repeating the ſame experiment.

In the 11th, 12th, 13th, and 14th experiments, when the piece was fired with 310 grains of powder and a bullet, the recoil was 24,69, 24,95, 24,9, and 24,9: and in the 15th, 16th, 18th, and 19th experimentss with 330 grains of powder, the recoil was 26,2, 26,2, 26,3, and 26,4. The regularity of theſe numbers is very ſtriking; and though we cannot compare the velocities of the bullets determined by the two methods as we have done in other caſes (as there are reaſons to believe, that the velocities, as they are ſet down in the tables, are not much to be depended on, and as the recoil, with the given charge of 310 grains of powder without a bullet, is not known) yet the regularity of the recoil in theſe experiments affords good grounds to conclude, that the method of determining the velocities of bullets founded upon it muſt be very accurate.

But of all the experiments thoſe numbered from 84 to 92 incluſive afford the ſtrongeſt proof of the accuracy of this method. In thoſe every poſſible precaution was taken to prevent errors ariſing from adventitious circumſtances, and the weights of the bullets and their velocities were ſo various, that the uniform agreement of the two methods of determining the velocities in thoſe trials amounts almoſt to a demonſtration of the truth of the principles upon which this new method is founded.

By the following table the reſult of theſe experiments may be ſeen at one view.

The experiments.	Weight of the bullets.	The barrel heavier than the bullet.	The recoil.	Velocity of the bullet.		Difference.
				By the recoil.	By the pendulum	
	Grs.	$\frac{W}{B}=$	$C=$	$v=$	$v=$	
84th and 93d	——	——	$c=$ 4,4	——	——	——
85th	90	3733,3	7,16	2109	1763	+346
86th	251	1338,6	9,62	1430	1317	+113
87th	354	949,15	11,03	1288	1136	+152
88th	600	560,	15,22	1240	1229	+ 11
89th	603	557,22	15,13	1224	1229	+ 5
90th	1184	283,78	21,92	1017	978	− 39
91ſt	1754	191,56	27,18	893	916	− 23
92d	2352	141,86	32,25	812	833	− 21

The charge of powder conſiſted of 145 grains in weight in each experiment.

In order to ſhew, in a more ſtriking manner, the reſult of theſe experiments, and the compariſon of the two methods of aſcertaining the velocities of bullets, I have drawn the fig. 16. where the numbers that are marked upon the line AB are taken from A towards B, in proportion to the weights of the bullets; while the lines drawn from thoſe numbers perpendicular from AB (as w, v, for inſtance, at the number 2352) and ending at the curve c, d, expreſs their velocities, as ſhewn by the pendulum. The continuation of thoſe lines on the oppoſite ſide of the line AB ſhew the recoil, and alſo the velocities of the bullets as determined from it; thus w, r, and the (dotted) lines parallel to it, which end at the line *g*, *f*, expreſs the recoil; and the portion of each of thoſe lines that is comprehended between the line AB and the curve *m*, *n* (as *w*, *u*) is as the velocity of the bullet in the ſeveral experiments. The line A, *e*, denotes the weight of the charge of powder; and the line A, *m*, the velocity with which the elaſtic fluid eſcapes out of the piece, when the powder is fired without any bullet.

Upon an inſpection of this figure, as well as from an examination of the foregoing table, it appears, that the velocities determined by the two methods agree with great nicety in all the experiments after the 87th; but in the 87th experiment, and alſo in the 86th, but particularly in the 85th, the difference in the reſult of theſe different methods is very conſiderable: and it is remarkable, that in thoſe experiments where they diſagree moſt, the velocities of the bullets, as determined by the pendulum, are extremely irregular; while, on the other hand, the gradual increaſe of the recoil as the bullets were heavier, and the great regularity of the correſponding velocities, afford good grounds to conclude, that this diſagreement is not owing to any inaccuracy in the new method of aſcertaining the velocities, but to ſome other cauſe that remains to be inveſtigated.

But before we proceed in this inquiry, let us ſeparate the five laſt experiments in the foregoing table; and, ſumming up the velocities determined by the two methods, we ſhall ſee by their difference how thoſe methods agreed upon the whole, in this inſtance.

Experiments.	Weight of the bullets.	Velocity.		Difference.
		By the recoil.	By the pendulum.	
	Grs.			
88th	600	1240	1229	+ 11
89th	603	1224	1229	− 5
90th	1184	1017	978	+ 39
91ſt	1754	893	916	− 23
92d	2352	812	833	− 21
Sums and diff. of the velocities		5186	5185	+ 1

Here the difference in the reſult of the two methods does not amount to $\frac{1}{5000}$th part of the whole velocity; but I lay no ſtreſs upon this extraordinary argument. I am ſenſible that it muſt in ſome degree have been accidental; but as the difference in the velocities, computed by theſe different methods, was in no inſtance conſiderable, not being in any caſe ſo great as what frequently occurred in the moſt careful repetition of the ſame experiment, and as the velocities, as determined by the recoil, were much more regular than thoſe ſhewn by the pendulum, as appears by comparing the curves *g*, *f*, and *m*, *n*, (fig. 16.) with the crooked line *c*, *d*, I think we may fairly conclude, that this new method may with ſafety be relied on in practice.

The greateſt difference in the velocities, as aſcertained by the two methods, appears, in the inſtance of the 85th experiment, where the velocity determined from the recoil exceeds that ſhewn by the pendulum by 346 feet in a ſecond, the former velocity being that of 2109 feet in a ſecond, the latter only 1763 feet in a ſecond; and in the two ſucceeding experiments, the velocities ſhewn by the pendulum are likewiſe deficient, though not in ſo great a degree.

This apparent deficiency remains now to be accounted for; and, firſt, it cannot be ſuppoſed, that it aroſe from any imperfection in Mr. ROBINS's method of determining the velocities of bullets; for that method is founded upon ſuch principles as leave no room to doubt of its accuracy; and the practical errors that occur in making the experiments, and which cannot be intirely prevented, or exactly compenſated, are in general ſo ſmall, that the difference of the velocities in queſtion cannot be attributed to them. It is true, the effect of thoſe errors is more likely to appear in experiments made under ſuch circumſtances as thoſe under which the experiments we are now ſpeaking of were made, than in any other caſe; for the bullets being very light, the arc of the aſcent of the pendulum was but ſmall, and a ſmall miſtake in meaſuring the chord upon the ribbon would have produced a very conſiderable error in computing the velocity of the bullet: thus, a difference of one tenth of an inch, more or leſs, upon the ribbon in the 85th experiment, would have made a difference in the velocity of more than 120 feet in a ſecond. But independent of the pains that were taken to prevent miſtakes, the ſtriking agreement of the velocities determined by the two methods in the experiments which immediately follow, as alſo in all other caſes where they could be compared, affords abundant reaſon to conclude, that the errors ariſing from thoſe cauſes were in no inſtance very conſiderable.

But if both methods of aſcertaining the velocities of bullets are to be relied on, then the difference of the velocities, as determined by them in theſe experiments, can only be accounted for by ſuppoſing that it aroſe from their having been diminiſhed by the reſiſtance of the air in the paſſage of the bullets from the mouth of the piece to the pendulum; and this ſuſpicion will be much ſtrengthened when we conſider how great the reſiſtance is that the air oppoſes to bodies that move very ſwiftly in it, and that the bullets in theſe experiments were not only projected with great velocities, but were alſo very light, and conſequently more liable to be retarded by the reſiſtance on that account.

To put the matter beyond all doubt, let us ſee what the reſiſtance was that theſe bullets met with, and how much their velocities were diminiſhed by it. The weight of the bullet (in the 85th experiment) was 90 grains; its diameter was 0,78 of an inch, and it was projected with a velocity of 2109 feet in a ſecond.

If now a computation be made according to the method laid down by Sir ISAAC NEWTON for compreſſed fluids, it will be found, that the reſiſtance to this bullet was not leſs than 8½ lbs. avoirdupois, which is ſomething more than 660 times its weight. But Mr. ROBINS has ſhewn, by experiment, that the reſiſtance of the air to bodies moving in it with very great velocity is near three times greater than Sir ISAAC has determined it, and as the velocity with which this bullet was impelled is conſiderably greater than any in Mr. ROBINS's experiments, it is highly probable, that the reſiſtance in this inſtance was at leaſt 2000 times greater than the weight of the bullet.

The diſtance from the mouth of the piece to the pendulum, as we have before obſerved, was 12 feet; but, as there is reaſon to think, that the blaſt of the powder, which always follows the bullet, continues to act upon it for ſome ſenſible ſpace of time after it is out of the bore, and by urging it on counter-balances, or at leaſt counter-acts in a great meaſure the reſiſtance of the air, we will ſuppoſe, that the reſiſtance does not begin, or rather that the motion of the bullet does not begin to be retarded, till it has got to the diſtance of two feet from the muzzle. The diſtance, therefore, between the barrel and the pendulum, inſtead of 12 feet, is to be eſtimated at 10 feet; and as the bullet took up about $\frac{1}{192}$ part of a ſecond in running over that ſpace, it muſt in that time have loſt a velocity of about 335 feet in a ſecond, as will appear upon making the computation, and this will very exactly account for the apparent diminution of the velocity in the experiment; for the difference of the velocities, as determined by the recoil and by the pendulum, $= 2109 - 1763 = 346$ feet in a ſecond, is extremely near 335 feet in a ſecond, the diminution of the velocity by the reſiſtance as here determined.

If the diminution of the velocities of the bullets in the two ſubſequent experiments be computed in like manner, it will turn out in the 86th experiment = 65 feet in a ſecond, and in the 87th experiment = 33 feet in a ſecond; and making theſe corrections, the compariſon of the two methods of aſcertaining the velocities will ſtand thus:

	85th exp.	86th exp.	87th exp.
Velocities ſhewn by the pendulum,	1763	1317	1136
Add the diminution of the velocity by the reſiſtance of the air,	335	65	33
	2098	1382	1169
Velocity by the recoil, -	2109	1430	1288
The difference =	+11	+48	+119

So that it appears, notwithſtanding theſe corrections, that the velocities in the 86th and 87th experiments, and particularly in the laſt, as they were determined by the pendulum, are ſtill conſiderably deficient. But the manifeſt irregularity of the velocities in thoſe inſtances affords abundant reaſon to conclude, that it muſt have ariſen from ſome extraordinary accidental cauſe, and therefore, that little dependance is to be put upon the reſult of thoſe experiments. I cannot take upon me to determine poſitively what the cauſe was which produced this irregularity; but I ſtrongly ſuſpect, that it aroſe from the breaking of the bullets in the barrel by the force of the exploſion: for theſe bullets, as has already been mentioned, were formed of lead, incloſing leſſer bullets of plaſter of Paris; and I well remember to have obſerved at the time ſeveral ſmall fragments of the plaſter which had fallen down by the ſide of the pendulum. I confeſs, I did not then pay much attention to this circumſtance, as I naturally concluded, that it aroſe from the breaking of the bullet in penetrating the target of the pendulum, and that the ſmall pieces of plaſter I ſaw upon the ground had fallen out of the hole by which the bullet entered. But if the bullets were not abſolutely broken in pieces in firing, yet, if they were conſiderably bruiſed, and the plaſter or a part of it were ſeparated from the lead, ſuch a change in their form might produce a great increaſe of the reſiſtance, and even their initial velocities might be affected by it, for their form being changed from that of a globe to ſome other figure, they might not fit the bore, and a part of the force of the charge might be loſt by the windage.

That this actually happened in the 87th experiment ſeems very probable, as the velocity with which the bullet was projected, as it was determined by the recoil, is conſiderably leſs in proportion in that experiment than in either of thoſe that precede it in that ſet, or in thoſe which follow it, as will appear upon inſpecting the curvature of the line *m, n,* fig. 16. But I forbear to inſiſt further upon this matter.

As I have made an allowance for the reſiſtance of the air in theſe experiments, it may be expected that I ſhould do it in all other caſes; but, I think, it will appear upon enquiry, that the diminution of the velocities of the bullets on that account was in general ſo inconſiderable that it might ſafely be neglected: thus, for inſtance, in the experiments with an ounce of powder, when the velocity of the bullet was more than 1750 feet in a ſecond, the diminution turns out no more than about 25 or 30 feet in a ſecond, though we ſuppoſe the full reſiſtance to have begun ſo near as two feet from the mouth of the piece; and in all caſes where the velocities were leſs, the effect of the reſiſtance was leſs in a much greater proportion: and even in this inſtance there is reaſon to think, that the diminution of the velocity as we have determined it is too great; for the flame of gun-powder expands with ſuch an amazing rapidity, that it is ſcarcely to be ſuppoſed but that it follows the bullet, and continues to act upon it more than two feet, or even four feet, from the gun, and when the velocity of the bullet is leſs, its action upon it muſt be ſenſible at a ſtill greater diſtance.

With 218 grains of powder the recoil appears to have been very uniform; but if the velocities of the bullets are determined from the recoil in the 40th and ſeven following experiments, when this charge was made uſe of, and from the recoil without a bullet in the 72d and 73d experiments, the velocities will turn out conſiderably too ſmall, as we ſhall ſee by making the computation.

The

		Vent at 0.			Vent at 1,3.
The recoil in the	40th exp. was	18,	and in the 43d exp. it was		18,3
	41ſt	17,71	-	44th	18,35
	42d	17,91	-	45th	18,35
	47th	8,1	-	46th	18,35
		4)71,72			4)73,35
	Means =	17,93	and		18,34

And in the 72d and 73d experiments the recoil, with the ſame charge without a bullet, was 8,72, and 8,47 = 8,595 at a medium, the velocities therefore turn out,

	Vent at 0.		Vent at 1,3.	
By the recoil	1105	-	1153	
inſtead of	1225	and	1276	as they were ſhewn by the pend.

The difference 120 and 123 feet in a ſecond amounts to near one twelfth part of the whole velocity.

This difference is undoubtedly owing to the recoil without a bullet being taken too great; for it is not only greater than it ought to be, in order that the velocities of the bullets may come out right; but it is conſiderably greater in proportion than the recoil with any other charge.

Thus, with 145 grains of powder the recoil was 4,4
with 165 grains - - it was 5,55
290 grains - - - 10,66
330 grains - - - 12,7
and with 437½ grains - - - it was 17,9

And if the recoil with 218 grains is determined from theſe numbers by interpolation, it comes out 7,5; and with that value for C the velocities of the bullets in the before mentioned experiments appear to be,

	Vent at 0.		Vent at 1,3.	
	1243	and	1283	by the recoil
which is extremely near	1225	and	1276	the velocities

ſhewn by the pendulum.

It is to be remembered, that the 72d and 73d experiments, from which we before determined the recoil with the given charge of powder without a bullet, were not made upon the ſame day with the experiments before mentioned; and it is well known, that the force of powder is different upon different days. And it is worthy of remark, that in thoſe two experiments the ſtrength of government powder appeared to be conſiderably the greateſt. I mention theſe circumſtances to ſhew the probability there is, that the recoil in thoſe experiments, from ſome unknown cauſe, was greater than it ought to have been, or rather than it would have been, had the experiments been made at the ſame time when the experiments with the bullets were made; or at any other time under the ſame circumſtances.

As this method of determining the velocities of the bullets did not occur to me till after I had finiſhed the courſe of my experiments, and had taken down my apparatus, I have not had an opportunity of aſcertaining the recoil, with and without a bullet, with that degree of preciſion that I could wiſh. If I had thought of it ſooner, or if I had recollected that paſſage in Mr. ROBINS's new Principles of Gunnery, where he ſays, "The part of the recoil, ariſing from the expanſion of "the powder alone, is found to be no greater when it impels "a leaden bullet before it, than when the ſame quantity is fired "without any wad to confine it:" I ſay, if that paſſage had occurred to me before it had been too late, I certainly ſhould have taken ſome pains to have aſcertained the fact; but as it is, I think, enough has been done to ſhew, that there is the greateſt probability that the velocities of bullets may in all caſes be determined by the recoil with great accuracy; and I hope ſoon to have it in my power to put the matter out of all doubt, and to verify this new method by a courſe of concluſive experiments which I am preparing for that purpoſe.

In the mean time I would juſt obſerve, that if this method ſhould be found to anſwer, when applied to muſket bullets, it cannot fail to anſwer equally well when it is applied to cannon balls and bomb ſhells of the largeſt dimenſions; and it is apprehended, that it will be much preferable to any method hitherto made public; not only as it may be applied indifferently to all kinds of military projectiles, and that with very little trouble or expence in making the experiment; but alſo becauſe by it the velocities with which bullets are *actually projected* are determined; whereas by the pendulum their velocities can only be aſcertained at ſome diſtance from the gun, and after they have loſt a part of their initial velocities by the reſiſtance of the air through which they are obliged to paſs to arrive at the pendulum.

At the trifling expence of ten or fifteen pounds an apparatus might be conſtructed that would anſwer for making the experiments with all the different kinds of ordnance in the Britiſh ſervice. The advantages that might be derived from ſuch a ſet of experiments are too obvious to require being mentioned,

Of a very accurate method of proving gun-powder.

All the *eprouvettes*, or powder-triers, in common uſe are defective in many reſpects. Neither the abſolute force of gun-powder can be determined by means of them, nor the comparative force of different kinds of it, but under circumſtances very different from thoſe in which the powder is made uſe of in ſervice.

As the force of powder ariſes from the action of an elaſtic fluid that is generated from it in its inflammation, the quicker the charge takes fire, the more of this fluid will be generated in any given ſhort ſpace of time, and the greater of courſe will be its effect upon the bullet. But in the common method of proving gun-powder, the weight by which the powder is confined is ſo great in proportion to the quantity of the charge, that there is time quite ſufficient for the charge to be all inflamed, even when the powder is of the ſloweſt compoſition, before the body to be put in motion can be ſenſibly removed from its place. The experiment, therefore, may ſhew which of two kinds of powder is the ſtrongeſt, when equal quantities of both are confined in equal ſpaces, and completely inflamed; but the degree of the inflammability, which is a property eſſential to the goodneſs of the powder, cannot by theſe means be aſcertained.

Hence it appears, how powder may anſwer to the proof, ſuch as is commonly required, and may nevertheleſs turn out very indifferent when it comes to be uſed in ſervice. And this, I believe, frequently happens; at leaſt I know complaints from officers of the badneſs of our powder are very common; and I would ſuppoſe that no powder is ever received by the

Board of Ordnance but ſuch as has gone through the eſtabliſhed examination, and has anſwered to the uſual teſt of its being of the ſtandard degree of ſtrength.

But though the common powder triers may ſhew powder to be better than it really is, they never can make it appear to be worſe than it is; it will therefore always be the intereſt of thoſe who manufacture that commodity to adhere to the old method of proving it. But the purchaſer will find his account in having it examined in a method by which its goodneſs may be aſcertained with greater preciſion.

The method I would recommend is as follows. A quantity of powder being provided, which, from any previous examination or trial, is known to be of a proper degree of ſtrength to ſerve as a ſtandard for the proof of other powder, a given charge of it is to be fired, with a fit bullet, in a barrel ſuſpended by two pendulous rods, according to the method before deſcribed; and the recoil is to be carefully meaſured upon the ribbon. And this experiment being repeated three or four times; or oftener if there is any difference in the recoil, the mean and the extremes of the chords may be marked upon the ribbon by black lines drawn acroſs it, and the word *proof* may be written upon the middle line; or if the recoil is uniform (which it will be to a ſufficient degree of accuracy, if care is taken to make the experiments under the ſame circumſtances) then the *proof mark* is to be made in that part of the ribbon to which it was conſtantly drawn out by the recoil in the different trials.

The recoil, with a known charge of ſtandard powder, being thus aſcertained and marked upon the ribbon, let an equal quantity of any other powder (that is to be proved) be fired in the ſame barrel, with a bullet of the ſame weight, and every other circumſtance alike, and if the ribbon is drawn out as far or farther than the proof mark, the powder is as good or better than the ſtandard; but if it falls ſhort of that diſtance, it is worſe than the ſtandard, and is to be rejected.

For the greater the velocity is with which the bullet is impelled, the greater will be the recoil; and when the recoil is the ſame, the velocities of the bullets are equal, and the powder is of the ſame degree of ſtrength, if the quantity of the charge is the ſame. And if care is taken in proportioning the charge to the weight of the bullet, to come as near as poſſible to the medium proportion that obtains in practice, the determination of the goodneſs of gun-powder from the reſult of this experiment cannot fail to hold good in actual ſervice.

Fig. 14. repreſents the propoſed apparatus, drawn to a ſcale of one foot to the inch. *a*, *b*, is the barrel ſuſpended by the pendulous rods *c*, *d*; and *r* is the ribbon for meaſuring the recoil.

The length of the bore is 30 inches, and its diameter is one inch, conſequently it is juſt 30 calibres in length, and will carry a leaden bullet of about 3 ounces.

The barrel may be made of gun metal, or of caſt iron as that is a cheaper commodity; but great care muſt be taken in boring it, to make the cylinder perfectly ſtrait and ſmooth, as well as to preſerve the proper dimenſions. Of whatever metal the barrel is made, it ought to weigh at leaſt 50 lbs. in order that the velocity of the recoil may not be too great; and the rods by which it is ſuſpended ſhould be five feet in length. The vent may be about one twentieth of an inch in diameter; and it ſhould be *bouched* or lined with gold, in the ſame manner as the touch-hole is made in the better kind of fowling pieces, in order that its dimenſions may not be increaſed by repeated firing.

The bullets ſhould be made to fit the bore with very little windage; and it would be better if they were all caſt in the ſame mould, and of the ſame parcel of lead, as in that caſe their weights and dimenſions would be more accurately the ſame, and the experiments would of courſe be more concluſive.

The ſtated charge of powder may be half an ounce, and it ſhould always be put up in a cartridge of very fine paper; and after the piece is loaded it ſhould be primed with other powder, firſt taking care to prick the cartridge by thruſting a priming wire down the vent.

As it appears, from ſeveral experiments made on purpoſe to aſcertain the fact, that ramming the powder more or leſs has a very ſenſible effect to increaſe or diminiſh the force of the charge; to prevent any inaccuracies that might ariſe from that cauſe, a ram-rod, ſuch as is repreſented fig. 15. may be made uſe of. It is to be made of a cylindric piece of wood in the ſame manner as ram-rods in general are made, but with the addition of a ring C, about one inch and a half, or two inches in diameter, which, being placed at a proper diſtance from the end (*a*) of the ram-rod that goes up into the bore, will prevent its being thruſt up too far. This ring may be made of wood, or of any kind of metal as ſhall be found moſt convenient. The other end of the ram-rod (*b*) may be 31 or 32 inches in length from the ring, and the extremity of it being covered with a proper ſubſtance, it may be made uſe of for wiping out the barrel after each experiment.

The machine (*f*) for the tape to ſlide through may be the ſame as is deſcribed by Dr. HUTTON in his account of his experiments on the initial velocities of cannon balls; as his method is much better calculated to anſwer the purpoſe than that propoſed and made uſe of by Mr. ROBINS. It will alſo be better for the axis of the pendulous rods to reſt upon level pieces of wood or iron, than for them to move in circular grooves: only care muſt be taken to confine them by ſtaples or ſome other contrivance, to prevent their ſlipping out of their places.

The trunnions, by means of which the barrel is connected with the pendulous rods, and upon which it is ſupported, ſhould be as ſmall as poſſible, in order to leſſen the friction; and for the ſame reaſon they ſhould be well poliſhed, as well as the grooves that receive them. They need not be caſt upon the barrel, but may be ſcrewed into it after it is finiſhed.

In making the experiments, regard muſt be had to the heat of the barrel, as well as to the temperature of the atmoſphere; for heat and cold, dryneſs and moiſture, have a very ſenſible effect upon gun-powder to increaſe or diminiſh its force. If therefore a very great degree of accuracy is at any time required, it will be beſt to begin by firing the piece two or three times merely to warm it; after which three or four experiments may be made with ſtandard powder, to determine anew the proof mark (for the ſtrength of the ſame powder is different upon different days); and when this is done, the experiments with the powder that is to be proved are to be made, taking care to preſerve the ſame interval of time between the firings, that the heat of the piece may be the ſame in each trial.

If all theſe precautions are taken, and if the bullets are of the ſame weight and dimenſions, powder may be proved by this method with much greater accuracy than has hitherto been done by any of the common methods made uſe of for that purpoſe.

Of the comparative goodneſs, or value, of powder of different degrees of ſtrength.

Let V denote the velocity of the bullet with the ſtronger powder, and put v equal to the velocity with the weaker, when the charges are equal, and the weight and dimenſions of the bullets are the ſame, and when they are diſcharged from the ſame piece. If the charge is augmented when the weaker powder is made uſe of, till the velocity of the bullet is increaſed from v to V, or becomes equal to the velocity with the given charge of the ſtronger powder, the *value* of the charges may then be ſaid to be equal; and conſequently the weaker powder is as much worſe than the ſtronger, or is of leſs value in proportion as the quantity of it required by the pound, to produce the given effect is greater.

But it is well known, that the velocities, with different quantities of the ſame kind of powder, are in the ſub-duplicate ratio of the weights of the charges. The charges, therefore, muſt be as the ſquares of the velocities, and conſequently the charge of the weaker powder muſt be to that of the ſtronger, when the velocities are equal, as VV is to vv. The weaker powder is therefore as much worſe than the ſtronger as VV is greater than vv; or the comparative goodneſs of powder, of different degrees of ſtrength, is as the ſquares of the velocities of the bullets when the charges are equal.

The mean velocity of the bullets, as ſhewn by the pendulum in the 104th and 105th experiments, when the piece was fired with 145 grains of government powder, was 894 feet in a ſecond; and with the ſame quantity of double proof * battle powder (experiment N° 106) the velocity was 990 feet in a ſecond. Now the ſquares of theſe velocities, which, as we juſt obſerved, meaſure the goodneſs of the powder, are to each other as 1 is to 1,2263, or nearly as 5 is to 6.

With 218 grains of government powder, the mean velocity in four experiments (*viz.* the 40th, 41ſt, 42d, and 43d) was 1225 feet in a ſecond; and in the experiment N° 107. when the ſame quantity of double proof battle powder was made uſe of, the velocity was 1380 in a ſecond; and $\overline{1225}^2$ is to $\overline{1380}^2$ as 1 is to 1,2691.

With 290 grains, or half the weight of the bullet in government powder in the 109th, 110th, 111th, and 112th experiments, the mean velocity of the bullet was 1444 feet in a ſecond; but with the ſame quantity of the battle powder (experiment N° 116.) the velocity was 1525 feet in a ſecond; $\overline{1444}^2$ is to $\overline{1525}^2$, as 1 is to 1,1153.

By taking a medium of theſe trials it appears, that double proof battle powder is better than government powder in proportion as 1,2036 is to 1, or nearly as 6 is to 5.

But if, inſtead of weighing the powder, we eſtimate the quantity of the charge by meaſurement, or the ſpace it occupies in the bore of the piece, the comparative ſtrength of battle powder will appear to be conſiderably greater, or its ſtrength will be to that of government powder nearly as 4 is to 3; for the grains of this better kind of powder being more compact and nearly of a ſpherical form, a greater weight of it will lie in any given ſpace than of government powder, which is formed more looſely, and of various and of very irregular figures.

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* This is called *battle* powder, not becauſe it is uſed in battle or in war; but from *Battle*, the name of a village in Kent, where that kind of powder is made.

Now the common price of double proof battle powder, as it is ſold by the wholeſale dealers in that commodity, is at the rate of £. 10 *per* cwt. net; which is juſt two ſhillings by the pound; while government powder is ſold at £. 5 5*s*. *per* hundred, or one ſhilling and $\frac{6}{10}$th of a penny *per* pound; but battle powder is better than government powder only in the proportion of 1,2036 to 1, or of one ſhilling and two pence to one ſhilling and $\frac{6}{10}$th of a penny; battle powder is therefore ſold at the rate of ten pence by the pound, or $41\frac{2}{3}$ *per cent.* dearer than it ought to be; or thoſe, who make uſe of it in preference to government powder, do it at a certain loſs of $41\frac{2}{3}$ *per cent.* of the money that the powder coſts them.

Of the relation of the velocities of bullets to their weights.

According to Mr. ROBINS's theory, when bullets of the ſame diameter, but different weights, are diſcharged from the ſame piece by the ſame quantity of powder; their velocities ſhould be in the reciprocal ſub-duplicate ratio of their weights; but as this theory is founded upon a ſuppoſition that the action of the elaſtic fluid, generated from the powder, is always the ſame in any and every given part of the bore when the charge is the ſame, whatever may be the weight of the bullet; and as no allowance is made for the expenditure of force required to put the fluid itſelf in motion, or for the loſs of it by the vent; it is plain that the theory is defective. It is true, Dr. HUTTON in his experiments found this law to obtain without any great error, and poſſibly it may hold good with ſufficient accuracy in many caſes; for it ſometimes happens that a number of errors or actions, whoſe operations have a contrary tendency, ſo compenſate each other, that their effects when united are not ſenſible. But when this is the caſe, if any one of the cauſes of error is removed, thoſe which remain will be detected.

When any given charge is loaded with a heavy bullet, more of the powder is inflamed in any very ſhort ſpace of time than when the bullet is lighter, and the action of the powder ought of courſe to be greater on that account; but then a heavy bullet takes up more time in paſſing through the bore than a light one, and conſequently more of the elaſtic fluid, generated from the powder, eſcapes by the vent and by windage. It may happen, that the augmentation of the force, on account of one of theſe circumſtances, may exactly counterbalance the diminution of it ariſing from the other; and if it ſhould be found upon trial that this is the caſe in general, in pieces as they are now conſtructed, and with all the variety of ſhot that are made uſe of in practice, it would be of great uſe to know the fact: and poſſibly it might anſwer as well, as far as it relates to the art of gunnery, as if we were perfectly acquainted with, and were able to appreciate, the effect of each varying circumſtance under which an experiment can be made. But when, concluding too haſtily from the reſult of a partial experiment, we ſuppoſe with Mr. ROBINS, that becauſe the ſum total of the action or preſſure of the elaſtic fluid upon the bullet, during the time of its paſſage through the bore, happens to be the ſame when bullets of different weights are made uſe of (which collective preſſure is in all caſes proportional to, and is accurately meaſured by, the velocity, or rather motion, communicated to the bullet) that therefore the preſſure in any given part is always exactly the ſame, when the quantity of powder is the ſame with which the piece is fired; and from thence endeavour to prove, that the inflammation of gun-powder is inſtantaneous, or that the whole charge is in all caſes inflamed, and "converted into an elaſtic fluid before the bullet is ſenſibly moved "from its place;" ſuch reaſonings and concluſions may lead to very dangerous errors.

It is undoubtedly true, that if the principles aſſumed by Mr. ROBINS with reſpect to the manner in which gun-powder takes fire, and the relation of the elaſticity of the generated fluid to its denſity, or the intenſity of its preſſure upon the bullet as it expands in the barrel, were juſt, and if the loſs of force by the vent and windage was in all caſes inconſiderable, or if it was prevented, the deductions from the theory reſpecting the velocities of bullets of different weights would always hold good. But if, on the contrary, it ſhould be found upon making the experiments carefully, and in ſuch a manner as intirely to prevent inaccuracies ariſing from adventitious circumſtances, that the velocities obſerve a law different from that which the theory ſuppoſes, we may fairly conclude, that the principles upon which the theory is founded are erroneous.

Let us now ſee how far theſe experiments differ from the theory. Thoſe numbered from 84 to 92 incluſive were made in ſuch a manner that no part of the force of the powder was loſt by the vent, or by windage, as has already been mentioned, and all poſſible attention was paid to every circumſtance that could contribute to render them perfect and concluſive.

A particular account of them with the means uſed for forming the bullets, and making them fit for the bore, and the contrivance for preventing the eſcape of the elaſtic fluid by the vent, &c. may be ſeen in the general table, p. 245. The following table ſhews the reſult of them.

N. B. The charge of powder was the ſame in each experiment, and conſiſted of 145 grains in weight.

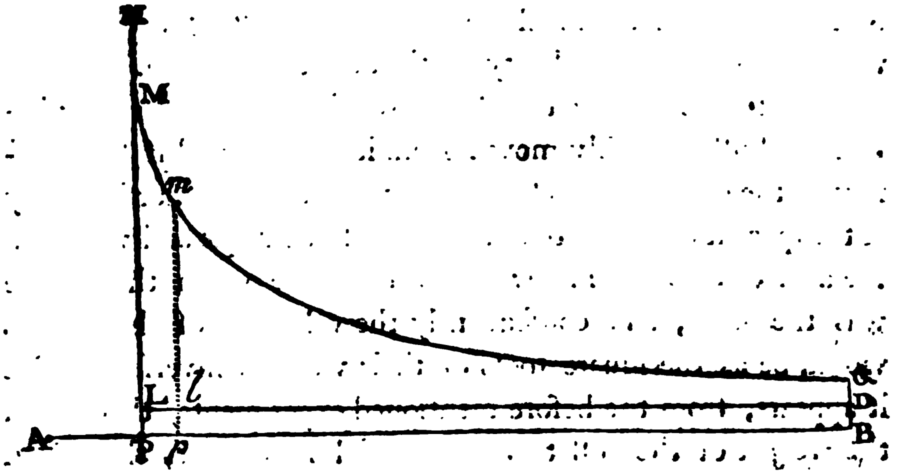
	Weight of the bullet.	Velocity of the bullet.		
		Actual.	Computed.	Difference.
85th exp.	90 grs.	2109	2109	——
86th	251	1430	1262	+168
87th	354	1288	1063	+225
88th	600	1240	817	+423
89th	603	1224	815	+409
90th	1184	1017	581	+436
91ſt	1754	893	478	+415
92d	2352	812	413	+399

The computed velocities, as they are ſet down in this table, were determined from the actual velocity of the bullet, as determined by the recoil in the 85th experiment; and the reciprocal ſub-duplicate ratio of its weight to the weight of the bullet in each ſubſequent experiment; and in the laſt column is marked the difference between the experiment and the theory, or the number of feet in a ſecond, by which the actual velocity exceeds the computed.

But in order that we may ſee this matter in different points of view, let the order of the experiments be now inverted, and let the computed velocities be determined from the actual velocity in the 92d experiment; and aſſuming the total or collective preſſure exerted by the powder upon the bullet in that experiment equal to unity, let the collective preſſure in the other experiments be computed from the ratio of the actual to the computed velocities, and the table will ſtand thus:

	Weight of the bullet.	Velocity of the bullet Actual.	 Computed.	Difference.	Collective preſſure.
92d exp.	2352 grs.	812	812	——	1,0000
91ſt	1754	893	940	− 47	0,9020
90th	1184	1017	1145	− 128	0,7897
89th	603	1224	1604	− 380	0,5825
88th	600	1240	1608	− 368	0,5949
87th	354	1288	2093	− 805	0,3778
86th	251	1430	2486	−1056	0,3310
85th	90	2109	4151	−2042	0,2581

In the following figure let AB repreſent the axis of the piece, and AP the length of the ſpace filled with powder; and at the



point P let the perpendicular PH be erected, upon which let PL and PM be taken from P towards H of ſuch magnitudes that while PL expounds the uniform force of gravity, or the weight of the bullet, PM ſhall be as the force exerted by the powder upon the bullet at the moment of the exploſion. If now we ſuppoſe, that while the bullet moves on from P towards B, the line PM or *pm*, goes along with it, and that the point *m* is always taken in ſuch a manner that the line *pm*, ſhall be to *pl*, or PL, as the force acting upon the bullet in the point *p*, is to its weight, till *pm*, coincides with QB, then will the area PMQB be to the area PLDB in the duplicate proportion of the velocities which the bullet would acquire when acted on by its own gravity through the ſpace PB, and when impelled through the ſame ſpace by the force of the powder, as may be ſeen demonſtrated by Sir ISAAC NEWTON, in his Mathematical Principles of Natural Philoſophy, book I. prop. 39.

Now what I call the collective preſſure, or ſum total of the action of the powder upon the bullet, is the meaſure of the area PMQB; and it is plain, from what has been ſaid above, that its meaſures are in all caſes to be accurately determined, when the weight and velocity of the bullet are known.

If all the powder of the charge was inflamed at once, or before the bullet ſenſibly moved from its place; and if the preſſure of the generated fluid was always as its denſity, or inverſely as the ſpace it occupies, then would the line MQ be an hyberbola, the area PMQB would always be the ſame when the charge was the ſame, and conſequently the velocities of the bullets would be as the ſquare roots of their weights inverſely. But it appears, from the before mentioned experiments, that when the weight of the bullet was increaſed four times, the action of the powder, or area PMQB, was nearly doubled; for in the 92d experiment, when four bullets were diſcharged at once, the collective preſſure was as 1; but in the 89th experiment, when a ſingle bullet was made uſe of, the collective preſſure was only as 0,5825; and in the 85th, 86th, and 87th experiments, when the bullets were much lighter, the action of the charge was ſtill leſs.

But though we can determine with great certainty, from theſe experiments, the ratio in which the action of the powder upon the bullet was increaſed or diminiſhed, by making uſe of bullets of greater or leſs weight; yet we cannot from thence aſcertain the relation of the elaſticity of the generated fluid to its denſity, nor the quantity of powder that is inflamed at different periods before and after the bullet begins to move in the bore.

But aſſuming Mr. ROBINS's principles as far as relates to the elaſticity of the fluid, and ſuppoſing that in all the experiments, except the 92d, a part only of the charge took fire, and that that part was inflamed and converted into an elaſtic fluid before the bullet began to move; upon that ſuppoſition we can determine the quantity of powder that took fire in each experiment; for the quantity of powder in that caſe would be as the collective preſſure.

Thus, if the whole charge, = 145 grains in weight, is ſuppoſed to have been inflamed in the 92d experiment, the quantity inflamed in each of the other experiments will appear to have been as follows; *viz.*

	Weight of the bullet.	Velocity of the bullet.	Collective preſſure.	Powder inflamed.
85th exp.	90 grs.	2109	0,2581	37 grs.
86th	251	1430	0,3310	48
87th	354	1288	0,3778	55
88th	600	1240	0,5949	86
89th	603	1224	0,5825	84
90th	1184	1017	0,7897	114
91ſt	1754	893	0,9020	131

But there are many reaſons to ſuppoſe, that the diminution of the action of the powder upon the bullet, when it is lighter, is not ſo much owing to the ſmalleſs of the quantity of powder that takes fire in that caſe as to the *vis inertiæ* of the generated fluid. It is true, that a greater portion of the charge takes fire when the bullet is heavy than when it is light, as I found in the very experiments of which I am now ſpeaking; but then the quantity of unfired powder in any caſe was much too ſmall to account for the apparent diminution of the force when light bullets were made uſe of.

If the elaſtic fluid, in the action of which the force of powder conſiſts, were infinitely fine, or if its weight bore no proportion to that of the powder that generated it; and if the groſs matter, or *caput mortuum*, of the powder remained in the bottom of the bore after the exploſion, then, and upon no other ſuppoſition, would the preſſure upon the bullet be inverſely as the ſpace occupied by the fluid: but it is evident that this can never be the caſe.

A curious ſubject for ſpeculation here occurs: how far would it be advantageous, were it poſſible, to diminiſh the ſpecific gravity of gun-powder, and the fluid generated from it, without leſſening its elaſtic force? It would certainly act upon very light bullets with greater force; but when heavy ones came to be made uſe of, there is reaſon to think, that, except extraordinary precaution was taken to prevent it, the greateſt part of the force would be loſt by the vent and by windage.

The velocity with which elaſtic fluids ruſh into a void ſpace is as the elaſticity of the fluid directly, and inverſely as its denſity; if, therefore, the denſity of the fluid generated from powder was four times leſs than it is, its elaſticity remaining the ſame, it would iſſue out at the vent, and eſcape by the ſide of the bullet in the bore, with nearly four times as great a velocity as it does at preſent; but we know from experiment that the loſs of force on thoſe accounts is now very conſiderable.

In the experiments N° 76. and 77. when the piece was fired with 145 grains of powder, the velocity of the bullets at a medium was 1040 feet in a ſecond; but in the 88th and 89th experiments, when the bullets were even heavier, and the piece was fired with the ſame quantity of powder, the mean velocity was 1232 feet in a ſecond. The difference = 192 feet in a ſecond, anſwers to a difference of force greater in the laſt experiments than in the firſt in the proportion of 14 to 10.

I know of no way to account for this difference, but by ſuppoſing that it was owing intirely to the eſcape of the elaſtic fluid by the vent, and by windage, in thoſe experiments where the vent was open, and the bullets were put naked into the piece.

An elaſtic bow, made of very light wood, will throw an arrow, and eſpecially a light one, with greater velocity than a bow of ſteel of the ſame degree of ſtiffneſs: but, for practice, I think it is plain, that gun-powder may *be ſuppoſed* to be ſo light as to be rendered intirely uſeleſs: and for ſome purpoſes it ſeems probable, that it would not be the worſe for being even heavier than it is now made. Vents are abſolutely neceſſary in fire-arms, and in large pieces the windage muſt be conſiderable, in order that the bullets, which are not always ſo round as they ſhould be, may not ſtick in the bore; and thoſe who have been preſent at the firing of heavy artillery and large mortars with ſhot and ſhells, muſt have obſerved, that there is a ſenſible ſpace of time elapſes between the lighting of the prime and the exploſion; and that, during that interval, the flame is continually iſſuing out at the vent with a hiſſing noiſe, and with a prodigious velocity, as appears by the height to which the ſtream of fire mounts up in the air. It is plain, that this loſs muſt be greater in proportion as the ſhot that is diſcharged is heavier; and I have often fancied, that I perceived a ſenſible difference in the time that elapſed between the firing of the prime and the exploſion, when bullets were diſcharged, and when the piece has been fired with powder only; the time being apparently longer in the former caſe than the latter.

Almoſt all the writers upon gun-powder, and particularly thoſe of the laſt century, gave different recipes for powder that is deſigned for different uſes. Thus the French authors mention *poudre à mouſquet*, *poudre ordinaire de guerre*, *poudre de chaſſe*, and *poudre d'artifice*; all of which are compoſed of ſalt petre, ſulphur, and charcoal, taken in different proportions. Is it not probable, that this variety in the compoſition of powder was originally introduced in conſequence of obſervation that one kind of powder was better adapted for particular purpoſes than another, or from experiments made on purpoſe to aſcertain the fact? There is one circumſtance that would lead us to ſuppoſe that that was the caſe.— That kind of powder which was deſigned for great guns and mortars was weaker than thoſe which were intended to be uſed in ſmaller pieces: for if there is any foundation for theſe conjectures, it is certain, that the weakeſt powder, or the heavieſt in proportion to its elaſtic force, ought to be uſed to impel the heavieſt bullets, and particularly in guns that are imperfectly formed, where the vent is large, and the windage very great.

I am perfectly aware, that an objection may here be made, viz. that the elaſtic fluid, which is generated from gun-powder, muſt be ſuppoſed to have the ſame properties very nearly, whatever may be the proportion of the ſeveral ingredients, and that therefore the only difference there can be in powder is, that one kind may generate more of this fluid, and another leſs; and that when it is generated, it acts in the ſame manner, and will alike eſcape, and with the ſame velocity, by any paſſage it can find. But to this I anſwer, though the fluid may be the ſame, as undoubtedly it is, and though its denſity and elaſticity may be the ſame in all caſes at the inſtant of its generation, yet in the exploſion, the elaſtic and un-elaſtic parts are ſo mixed and blended, that I imagine the fluid cannot expand without taking the groſs matter along with it, and the velocity with which the flame iſſues out at the vent is to be computed from the elaſticity of the fluid, and the denſity or weight of the fluid and the groſs matter taken together, and not ſimply from the elaſticity and denſity of the fluid. If antimony in an impalpable powder, or any other heavy body, was intimately mixed with water in a veſſel of any kind, and kept in ſuſpenſion by ſhaking or ſtirring them about; and if a hole was opened in the ſide or bottom of the veſſel, the water would not run out without taking the particles of the ſolid body along with it. And in the ſame manner I conceive the ſolid particles that remain after the exploſion of gun-powder are carried forward with the generated elaſtic fluid, and being carried forward retard its motion.—But to return from this digreſſion.

As it appears from theſe experiments, that the relation of the velocities of bullets to their weights is different from that which Mr. ROBINS's theory ſuppoſes, it remains to inquire what the law is which actually obtains. And, firſt, as the velocities bear a greater proportion to each other than the reciprocal ſub-duplicate ratio of the weights of the bullets, let us ſee how near they come to the reciprocal ſub-triplicate ratio of their weights.

	Weight of the bullet.	Velocity of the bullet Computed. Recip. ſub-dup. ratio.	 Error of the theory.	 Actual.	 Error of the theory.	 Computed. Recip. ſub-trip. ratio.
92d	2352	812	——	812	——	812
91ſt	1754	940	+ 47	893	+ 2	895
90th	1184	1145	+ 128	1017	+ 4	1021
89th	603	1604	+ 380	1224	+ 54	1278
88th	600	1608	+ 368	1240	+ 40	1280
87th	354	2093	+ 805	1288	+ 239	1527
86th	251	2486	+ 1056	1430	+ 282	1712
85th	90	4151	+ 2042	2109	+ 301	2410

Here the velocities computed upon the laſt ſuppoſition appear to agree much better with the experiments than thoſe computed upon Mr. ROBINS's principles; but ſtill there is a conſiderable difference between the actual and the computed velocities in the three laſt experiments in the table.

As the powder itſelf is heavy, it may be conſidered as a weight that is put in motion along with the bullet; and if we ſuppoſe the denſity of the generated fluid is always uniform from the bullet to the breech, the velocity of the center of gravity of the powder or (which amounts to the ſame thing) of the elaſtic fluid, and the groſs matter generated from it will be juſt half as great as the velocity of the bullet. If therefore we put P to denote the weight of the powder, B the weight of the bullet, and v its initial velocity; then $Bv + \frac{1}{2}Pv = \overline{B+\frac{1}{2}P} \times v$ will expreſs the *momentum* of the charge at the inſtant when the bullet quits the bore.

If now, inſtead of aſcertaining the relation of the velocities to the weights of the bullets, we add half the weight of the powder to the weight of the bullet, and compute the velocities from the reciprocal ſub-triplicate ratio of the quantity $\overline{B+\frac{1}{2}P}$ in each experiment, the table will ſtand thus:

	Weight of the bullet and half the powder. $\overline{B+\frac{1}{2}P}=$	Velocity of the bullet. Actual.	 Computed.	Error of the theory.
92d exp.	$\overline{2352+72\frac{1}{2}}$	812	812	——
91ſt	$\overline{1754+72\frac{1}{2}}$	893	892	− 1
90th	$\overline{1184+72\frac{1}{2}}$	1017	1011	− 6
89th	$\overline{603+72\frac{1}{2}}$	1224	1243	+ 19
88th	$\overline{600+72\frac{1}{2}}$	1240	1245	+ 5
87th	$\overline{354+72\frac{1}{2}}$	1288	1449	+161
86th	$\overline{251+72\frac{1}{2}}$	1430	1589	+159
85th	$\overline{90+72\frac{1}{2}}$	2109	1999	−110

The agreement between the actual and computed velocities is here very remarkable, and particularly in the five first experiments, which are certainly thoſe upon which the greateſt dependence may be placed.

And hence we are enabled to determine the natures of the *mn*, and *gf* (fig. 16); for ſince B (which expreſſes the weight of the bullet) is as the length taken from A towards B in the figure in the ſeveral experiments; and as the velocities are as the lines drawn perpendicular to the line AB from the places where thoſe lengths terminate, as *w*, *n*, &c. ending at the curve *m*, *n*; if we put $a = \frac{1}{2}P$, $x = B$, and $y = wr$; then will the relation of x and y be defined by this equation $\frac{1}{\sqrt{a+x^3}} = y$. And if z be put to denote the line $w\,r$, and b, the recoil when the given charge is fired without any bullet, it will be $\frac{x}{\sqrt{a+x^3}} + b = z$ in the curve *gf*, x being the abſciſſa, and z the correſponding ordinate to the curve.

In the 92d experiment half the weight of the powder $(= a)$ was 72½ grains; the weight of the bullet was 2352 grains $(= x)$; the recoil $(= z)$ was 32,25 inches; and with the given charge without any bullet the recoil $(= b)$ was 4,4 inches; if now from theſe *data*, and the known weight of the bullet in each of the other experiments in this ſet, the recoil be computed by means of the theorem $\frac{x}{\sqrt{a+x^3}} + b = z$, we ſhall ſee how the reſult of thoſe experiments agrees with this theory thus,

	Weight of the bullet.	Receil Actual.	Receil Computed.	Difference.
92d exp.	2352	32,25	32,25	——
91ſt	1754	27,18	27,22	+0,04
90th	1184	21,92	21,85	−0,07
89th	603	15,13	15,33	+0,2
88th	600	15,22	15,29	+0,07
87th	354	11,03	11,87	+0,84
86th	251	9,62	10,21	+0,59
85th	90	7,16	7,02	−0,14
84th and 93d	0	4,4	4,4	——

Here the agreement of the actual and computed receils is as remarkable as that of the actual and computed velocities in the foregoing table.

By the figure 17. may be ſeen at one view the reſult of all theſe experiments and computations. The numbers upon the line AB (as in the fig. 16.) repreſent the weights of the bullets, while the lines drawn from thoſe numbers perpendicular to AB on each ſide, and ending at the curves m, n, are as the velocities of the bullets in the ſeveral experiments; the line AB being the axis of the curves, the lengths taken from A to the different numbers towards B ($=x$) the abſciſſa, and the perpendiculars ($=y$) the correſponding ordinates. The ordinates to the the curve bn, are as the velocities computed from the theorem $\frac{1}{\sqrt{a+x^2}}=y$, and the ordinates to the curve p, n (which is the logarithmic curve, as it is $\frac{1}{\sqrt{x}}=y$) ſhew the velocities computed upon Mr. ROBINS's principles. The curve gf is drawn from the theorem $\frac{x}{\sqrt{a+x^3}}+b=z$; and the actual recoil is marked upon the ordinates to this curve by large round dots, which in all the experiments, except the 86th and 87th, very nearly coincide with the curve.

In the fig. 18. the numbers upon the line AB, taken from A, denote the different charges of powder uſed in the courſe of the experiments, while the ordinates to the curve *cd*, expreſs the velocities of the bullets, with the vent at o. The lines drawn perpendicular from the line AB to the line *ef*, repreſent the recoil with the ſeveral charges of powder, and a leaden bullet; and the portion of thoſe lines that is comprehended between the line AB and the line *gh*, denotes the recoil when the given charge was fired without any bullet.

Having now ſhewn by experiment the relation of the velocities of bullets to their weights, when care is taken to prevent intirely the loſs of force by the eſcape of the elaſtic fluid through the vent and by the windage, I ſhall leave it to mathematicians to determine from theſe *data* the properties of that fluid.

But, before I take my leave of this ſubject, I would juſt obſerve, that Mr. ROBINS is not only miſtaken in the principle he aſſumes, reſpecting the relation of the elaſticity of the fluid generated from gun-powder to its denſity, or rather the law of its action upon the bullet as it expands in the bore; but his determination of the force of gun-powder is alſo erroneous, even upon his own principles: for he determines its force to be 1000 times greater than the mean preſſure of the atmoſphere; whereas it appears, from the reſult of the 92d experiment, that its force is at leaſt 1308 times greater than the mean preſſure of the atmoſphere, as will be evident to thoſe who will take the trouble to make the computation.

Of an attempt to determine the exploſive force of aurum fulminans, *or a compariſon between its force and that of gun-powder.*

Having provided myſelf with a ſmall quantity of this wonderful powder, upon the goodneſs of which I could depend, I endeavoured to aſcertain its exploſive force by making uſe of it inſtead of gun-powder for diſcharging a bullet, and meaſuring, by means of the pendulum, the velocity which the bullet acquired; and concluding, from the tremendous report with which this ſubſtance explodes, that its elaſtic force was vaſtly greater than that of gun-powder, I took care to have a barrel provided of uncommon ſtrength, on purpoſe for the experiment. Its length in the bore is 13,25 inches, the diameter of the bore is 0,55 of an inch, and its weight 7 lbs. 2 oz. It is of the beſt iron, and was made by WOGDON; and the accuracy with which it is finiſhed does credit to the workman.

This barrel being charged with one ſixteenth of an ounce (= 27,34 grains) of *aurum fulminans* and two leaden bullets, which, together with the leather that was put about them to make them fit the bore without windage, weighed 427 grains; it was laid upon a chaffing-diſh of live coals, at the diſtance of about 10 feet from the pendulum, and againſt the center of the target of the pendulum the piece was directed.

Having ſecured the barrel in ſuch a manner that its direction ſhould not alter, I retired to a little diſtance, in order to be out of danger in caſe of an accident, where I waited in anxious expectation the event of the exploſion.

I had remained in this ſituation ſome minutes, and almoſt deſpaired of the experiment's ſucceeding, when the powder exploded, but with a report infinitely leſs than what I expected, the noiſe not greatly exceeding the report of a well-charged wind gun; and it was not till I ſaw the pendulum in motion that I could be perſuaded that the bullets had been diſcharged. I found, however, upon examination, that nothing was left in the barrel, and from the great number of ſmall particles of revived metal that were diſperſed about, I had reaſon to think that all the powder had exploded.

The bullets ſtruck the pendulum nearly in the center of the target, and both of them remained in the wood: and I found, upon making the calculation, that they had impinged againſt it with a velocity of 428 feet in a ſecond.

If we now ſuppoſe that the force of *aurum fulminans* ariſes from the action of an elaſtic fluid that is generated from it in the moment of its exploſion, and that the elaſticity of this fluid, or rather the force it exerts upon the bullet as it goes on to expand, is always as its denſity, or inverſely as the ſpace it occupies; then, from the known dimenſions of the barrel, the length of the ſpace occupied by the charge (which in this experiment was 0,47 of an inch), and the weight and velocity of the bullets, the elaſtic force of this fluid at the inſtant of its generation may be determined: and I find, upon making the calculation upon theſe principles, that its force turns out 307 times greater than the mean elaſtic force of common air.

According to Mr. ROBINS's theory, the elaſtic force of the fluid generated from gun-powder in its inflammation is 1,000 times greater than the mean preſſure of the atmoſphere; the force of *aurum fulminans*, therefore, appears to be to that of gun-powder as 307 is to 1,000, or as 4 is to 13 very nearly.

Of the ſpecific gravity of gun-powder.

To determine the ſpecific gravity of gun-powder I made uſe of the following method. A large glaſs bucket, with a narrow mouth, being ſuſpended to one of the arms of a very nice balance, and exactly counter-poiſed by weights put in the oppoſite ſcale, it was filled firſt with government powder poured in lightly, then with the ſame powder ſhaken well together, afterwards with powder and water together, and laſtly with water alone, and in each caſe the contents of the bucket were very exactly weighed.

The ſpecific gravity of gun-powder, as determined from theſe experiments, is as follows:

Specific gravity of rain water	- -	1,000
Government powder, as it lies light in a heap, mixed with air	- - - -	0,836
Government powder well ſhaken together	-	0,937
The ſolid ſubſtance of the powder	- -	1,745

Hence it appears, that a cubic inch of government powder ſhaken well together weighs just 243 grains; that a cubic inch of ſolid powder would weigh 442 grains; and, conſequently, that the interſtices between the particles of the powder, as it is grained for uſe, are nearly as great as the ſpaces which theſe particles occupy.

MIS-

MISCELLANEOUS EXPERIMENTS.

Of ſome unſucceſsful attempts to increaſe the force of gun-powder.

It has been ſuppoſed by many, that the force of ſteam is even greater than that of gun-powder; and that if a quantity of water, confined in the chamber of a gun, could at once be rarified into ſteam, it would impel a bullet with prodigious velocity. Several attempts have been made to ſhoot bullets in this manner; but I know of none that have ſucceeded; at leaſt ſo far as to render it probable that water can ever be ſubſtituted in the room of gun-powder for military purpoſes, as ſome have imagined.

The great difficulty that attends making theſe experiments lies in finding out a method by which the water can at once be rarified, and converted into elaſtic ſteam; and it occurred to me, that poſſibly that might be effected by means of gun-powder, by confining a ſmall quantity of water in ſome very thin ſubſtance, and ſurrounding and incloſing it with powder, and afterwards ſetting fire to the charge. The method I took to do this was as follows. Having procured a number of air bladders of very ſmall fiſhes, I put different quantities of water into them, from the ſize of a ſmall pea to that of a ſmall piſtol bullet, and tying them up cloſe with ſome very fine thread, I hung up theſe little globules in the open air till they were quite dry on the outſide. I then provided a number of cartridges made of fine paper, and filled them with a known quantity of powder, equal to the cuſtomary charge for a common horſeman's piſtol; and having loaded ſuch a piſtol with one of them and a fit bullet, I laid it down upon the ground, and directing it againſt an oaken plank that was placed about ſix feet from the muzzle, I fired it off by a train, and carefully obſerved the recoil, and alſo the penetration of the bullet. I then took ſeveral of the filled cartridges that remained, and pouring out part of the powder, I put one or more of the little bladders filled with water in the center of the cartridge, and afterwards pouring back the remaining part of the charge, confined the water in the midſt of the powder.

With theſe cartridges and a fit bullet, the piſtol was ſucceſſively loaded, and being placed upon the ground as before, and fired by a train, the recoil, and the penetration of the bullets were obſerved; and I conſtantly found, that the force of the charge was very ſenſibly diminiſhed by the addition of the globule of water, and the larger the quantity of water was that was thus confined, the leſs was the effect of the charge; neither the recoil of the piſtol, nor the penetration of the bullet, being near equal to what they were when the given quantity of powder was fired without the water; and the report of the exploſion appeared to be leſſened in a ſtill greater proportion than the recoil or penetration.

Concluding that this diminution of the force of the charge aroſe from the burſting of the little bladder, and the diſperſion of the water among the powder before it was all inflamed, by which a great part of it was prevented from taking fire, I repeated the experiments with highly rectified ſpirits of wine inſtead of water; but the reſult was nearly the ſame as before: the force of the charge was conſtantly and very ſenſibly diminiſhed. I afterwards made uſe of etherial oil of turpentine, and then of ſmall quantities of quickſilver; but ſtill with no better ſucceſs. Every thing I mixed with the powder, inſtead of increaſing, ſerved to leſſen the force of the charge.

Theſe trials were all made ſeveral months before I began the courſe of my experiments upon gun-powder, which I have already given an account of; and though they were altogether unſucceſsful, yet I reſumed the inquiry at that time, and made ſeveral new experiments, with a view to find out ſomething that ſhould be ſtronger than powder, or which, when mixed with it, ſhould increaſe its force.

It is well known, that the elaſtic force of quickſilver converted into vapour is very great; this ſubſtance I made uſe of in my former trials, as I have juſt obſerved, but without ſucceſs. I thought, however, that the failure of that attempt might poſſibly be owing to the quickſilver being too much in a body, by which means the fire could not act upon it, to the greateſt advantage; but that if it could be divided into exceeding ſmall particles, and ſo ordered that each particle might be completely ſurrounded by, and expoſed to, the action of the flame of the powder, it would be very ſoon heated, and poſſibly might be converted into an elaſtic ſteam or vapour, before the bullet could be ſenſibly removed from its place. To determine this point I mixed 20 grains of æthiops mineral very intimately with 145 grains of powder, and charging the piece with this compound, it was loaded with a fit bullet and fired; but the force of the charge was leſs than that which the powder alone would have exerted, as appears by comparing the 76th and 77th experiments with the 79th.

Common *pulvis fulminans* is made of one part of ſulphur, two parts of ſalt of tartar, and three parts of nitre; and if we may judge by the report of the exploſion, the elaſtic force of this compound is conſiderably greater than that of gun-powder.

I was willing to ſee the effect of mixing ſalt of tartar with gun-powder, and accordingly having provided ſome of this alkaline ſalt in its pureſt ſtate, thoroughly dry, and in a fine powder, I mixed 20 grains of it with 145 grains of gun-powder; and upon diſcharging a bullet with the mixture, I found that the alkaline ſalt had conſiderably leſſened the force of the powder. See experiment N° 78.

I next made uſe of *ſal ammoniacum*. That ſalt has been found to produce a very large quantity of elaſtic air, or vapour, when expoſed to heat under certain circumſtances; but when 20 grains of it were mixed with a charge of gun-powder, inſtead of adding to its force, it diminiſhed it very ſenſibly. See the 80th experiment.

Moſt, if not all, the metals, are thought to produce large quantities of air when they are diſſolved in proper *menſtrua*, and particularly braſs, when it is diſſolved in ſpirit of nitre. Deſirous of ſeeing if this could be done by the flame, or acid vapour of fired powder, I mixed 20 grains of braſs in a very fine powder, commonly called braſs duſt (being the ſmall particles of this metal that fly off from the wheel in ſharpening pins), with 145 grains of powder, and with this compound and a fit bullet I loaded my barrel and diſcharged it; but the experiment (N° 81.) ſhewed, that the force of the powder was not increaſed by the addition of the braſs duſt, but the contrary.

It ſeems probable, however, that neither braſs duſt nor æthiops mineral are of themſelves capable of diminiſhing the force of gun-powder in any conſiderable degree, otherwiſe than, by filling up the interſtices between the grains, and obſtructing the paſſage of the flame, and ſo impeding the progreſs of the inflammation. And hence it appears, how earthy particles and impurities of all kinds are ſo very detrimental to gun-powder. It is not that they deſtroy or alter the properties of any of the bodies of which the powder is compoſed, but ſimply, that by obſtructing the progreſs of the inflammation, they leſſen its force, and render it of little or no value. Too much care, therefore, cannot be taken in manufacturing powder to free the materials from all heterogeneous matter.

Of an attempt to ſhoot flame inſtead of bullets.

Having often obſerved paper and other light bodies to come out of great guns and ſmall arms inflamed, I was led to try if other inflammable bodies might not be ſet on fire in like manner, and particularly inflammable fluids; and I thought if this could be effected, it might be poſſible to project ſuch ignited bodies by the force of the exploſion, and by that means communicate the fire to other bodies at ſome conſiderable diſtance; but in this attempt I failed totally. I never could ſet dry tow on fire at the diſtance of five yards from the muzzle of my barrel. I repeatedly diſcharged large wads of tow and paper, thoroughly ſoaked in the moſt inflammable fluids, ſuch as *alkohol*, *ætherial ſpirit of turpentine*, *balſam of ſulphur*, &c.; but none of them were ever ſet on fire by the exploſion. Sometimes I diſcharged three or four ſpoonfuls of the inflammable fluid, by interpoſing a very thin wad of cork over the powder, and another over the fluid; but ſtill with no better ſucceſs. The fluid was projected againſt the wall as before, and left a mark where it hit; but it never could be made to take fire; ſo I gave up the attempt. If it had ſucceeded, probably it would have turned out one of the moſt important diſcoveries in the art of war that have been made ſince the invention of gun-powder.

* * *

Fig. 1.

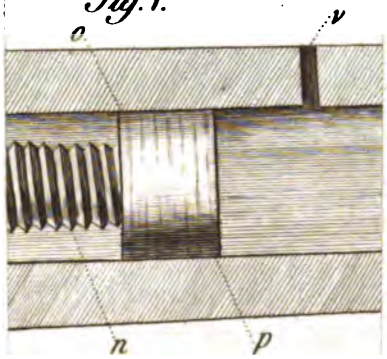


Fig. 4.

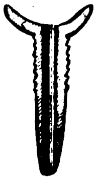


Fig. 2.

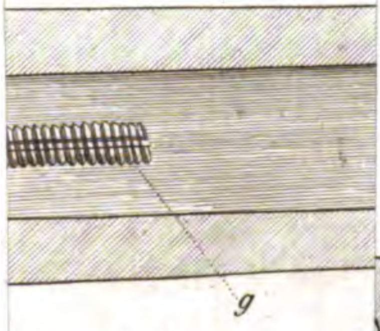
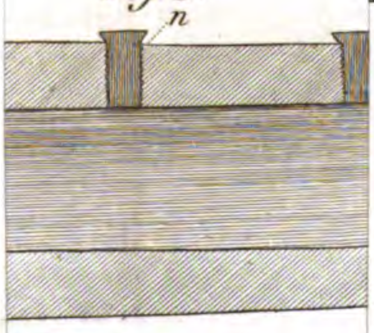


Fig. 3.



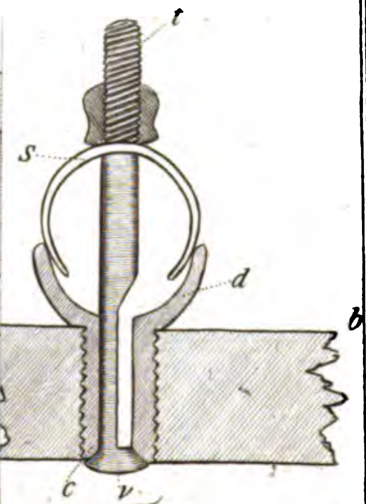


Fig. 5.

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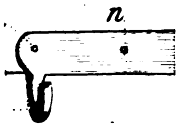

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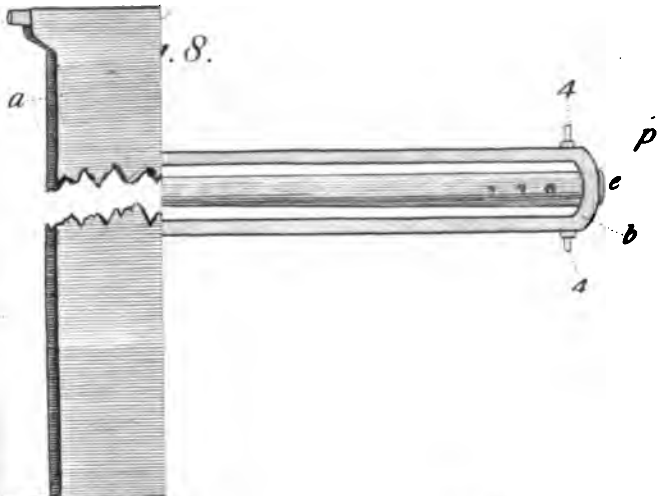
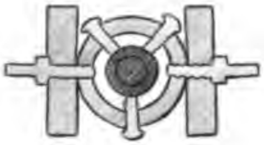


Fig. 10.



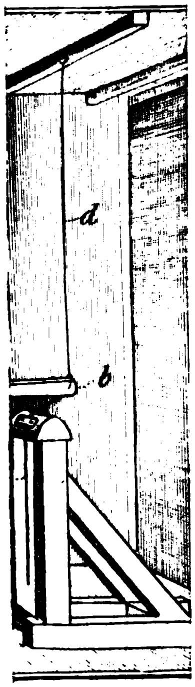
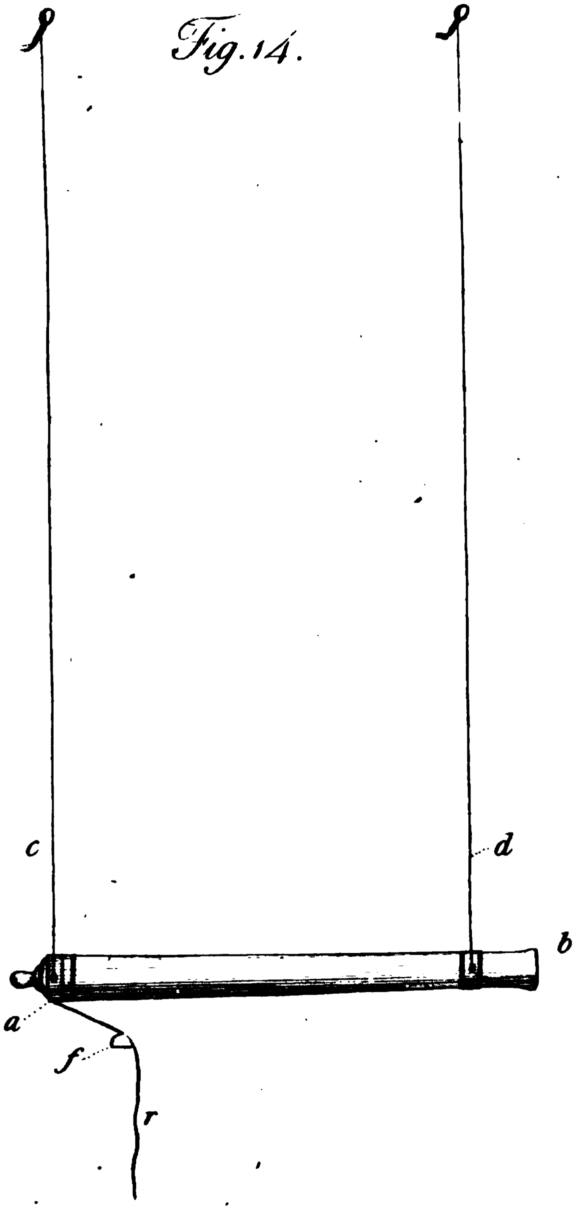


Fig. 14.



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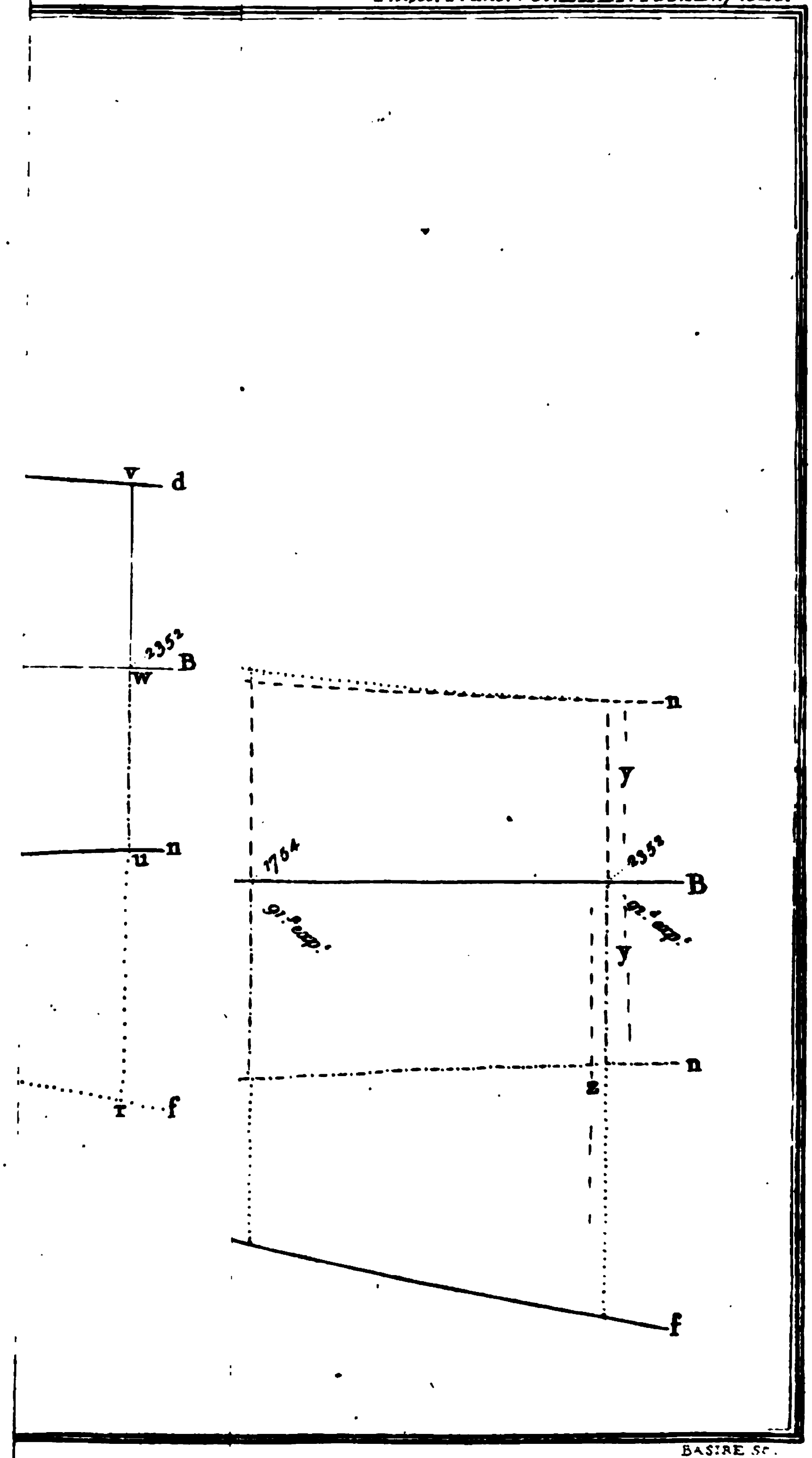

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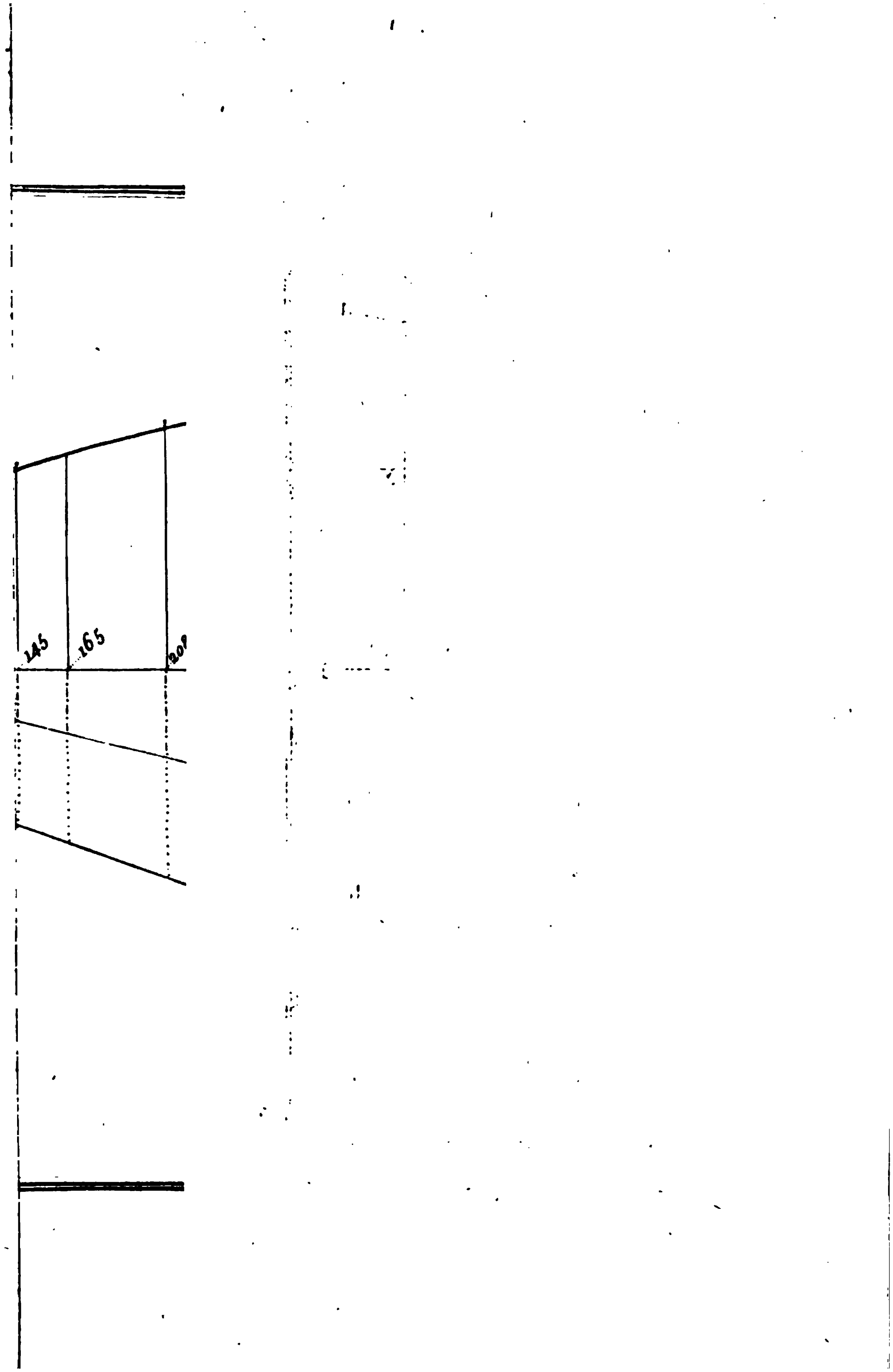


XVI. *Account of a luminous Appearance in the Heavens. By Mr.* Tiberius Cavallo, *F. R. S. in a Letter to Sir* Joſeph Banks, *Bart. P. R. S.*

Read April 5, 1781.

I TAKE the liberty to ſend you an account of a luminous appearance obſerved laſt night in the heavens, which ſeems to be very ſingular in its nature, and quite diſtinct from the *aurora borealis.*

At about half an hour paſt nine yeſterday evening, being the 27th of March, a white light began to be ſeen in the ſky, which became gradually more and more denſe till ten o'clock, at which time it formed a compleat luminous arch from eaſt to weſt. Of this I have been informed by others; but at a quarter paſt ten I went out of the houſe and obſerved it myſelf. At that time it appeared to be an arch of about ſeven or eight degrees in breadth extended from eaſt to weſt, or, as ſome of my friends imagined, in the direction of eaſt by north to weſt by ſouth. Its weſtern part quite reached the horizon; but the eaſtern part of the arch ſeemed to begin at about 50° or 60° above the horizon. It did not paſs through the zenith but at about 8° or 10° ſouthward of it, and it was nearly perpendicular to the horizon.

The whiteneſs of this arch was much denſer than that any *aurora borealis* I ever obſerved, though it did not caſt ſo much light upon the terreſtrial objects. Towards the middle it was ſo denſe, that the ſtars over which it paſſed were eclipſed; but the ſides of this luminous arch were more faint and tranſparent.

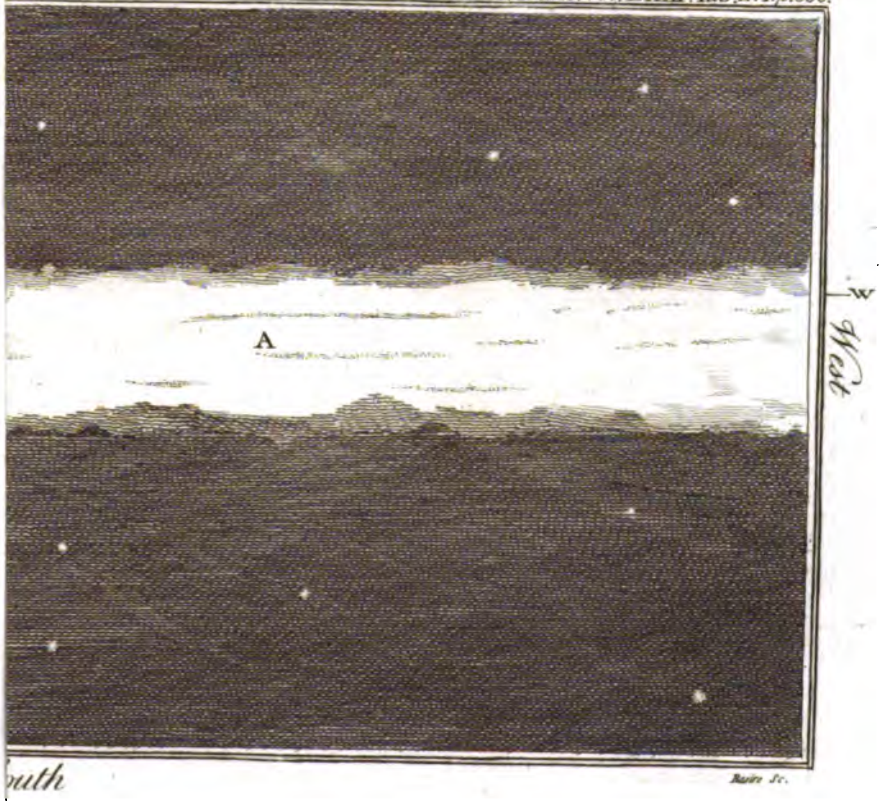
The incloſed drawing ſhews its appearance at about half an hour paſt ten o'clock. At about three quarters paſt ten it began to loſe its brightneſs, firſt at A, and then vaniſhed gradually, ſo that at eleven o'clock none of it could be perceived. As ſoon as any part of this arch loſt its denſe whiteneſs, the ſtars appeared through it quite diſtinct, ſo that it could not be a cloud. The light alſo ſeemed to vaniſh without change of place; for it did not appear to be diſperſed through the ſky, or to be driven in any direction.

This extraordinary appearance to me ſeemed quite diſtinct from the *aurora borealis* for the following reaſons, *viz.* becauſe it eclipſed the ſtars over which it paſſed; becauſe its light, or rather its white appearance, was ſtationary and not lambent; and becauſe its direction was from eaſt to weſt.

The atmoſphere was in other reſpects very ſerene, the ſtars ſhining very bright, and no cloud appearing. The northern light was exceedingly faint, and very low about the northern point of the horizon. The wind was nearly north-eaſt, and it could be juſt perceived in the ſtreets.

I am, &c,

Philos. Trans. Vol. LXXI. Tab. XVI. p. 330.



XVII. *Account of an Earthquake at* Hafodunos *near* Denbigh. *By* John Lloyd, *Eſq. F. R. S. in a Letter to Sir* Joſeph Banks, *Bart. P. R. S.*

Read April 5, 1781.

Hafodunos near Denbigh,
Dec. 31, 1781.

DEAR SIR,

UPON the 29th day of laſt Auguſt, at 8 h. 37′ 30″, I was ſitting on my * bed-ſide, and heard a rumbling noiſe, as if at a diſtance: the ſound ſeemed to approach me, and when it was greateſt the bed rocked and ſhook ſo much that I could ſcarcely keep my ſeat. I could have no doubt of its being an earthquake, and inſtantly looked at my barometer, which is of Mr. DE LUC's conſtruction; it ſtood at 29.57 inches. Attached thermometer 65°. The barometer had been ſtationary nearly for the three preceding days, and did not ſeem to be affected with the ſhock. The morning was remarkably fine, and not a ſingle cloud to be ſeen. Two of my ſiſters and a gentleman were walking upon the terrace in the garden by the ſide of a wall: they all perceived the noiſe, at firſt as if at a great diſtance; but when it was greateſt they perceived the wall to ſhake, though they did not obſerve any agitation under their feet. As they were walking, and obſerved the ſpot when they firſt heard the ſound, and the ſpot they came to when it ceaſed, I was enabled to aſcertain its duration pretty exactly, and find it to have been from fifteen to eighteen ſeconds. Its courſe was nearly from ſouth-eaſt to north-weſt. Some other perſons in our houſe perceived a double ſhock; and this has been obſerved by many who felt it in other places. It was felt at Flint by Mrs. SEAMAN and her daughter, who obſerved the cups rattled upon the ſaucers as they ſat at breakfaſt. Mr. PENNANT's family, at Downing, fancied that an empty waggon was coming into the back-court, which is paved. It was ſtrongly felt at Llonrwſt by the whole town, and part of a ſtone wall was flung down. At Carnarvon (which is in the ſame parallel of latitude as this place, 53° 10′) the ſhock was very ſlight. It was perceived in many places about Conway; but not at all by any one in town. Sir HUGH WILLIAMS felt it very ſtrongly at his houſe near Beaumaris. At our friend Mr. DAVIES's, in that town, a door clapped backwards and forwards ſeveral times; and at Lord BULKELEY's ſeat, Boron-hill, the family were much alarmed, it was ſo violent. It was ſtrongly felt at Holyhead, and at an eminent ſolicitor's in the iſland of Angleſey, the deſks before ſeveral clerks in his office ſhook ſo that they could not write. It was ſtrongly perceived at Mr. FITZMAURICE's, at Lleweny-hall in the Vale of Clwyd, and in ſeveral other places in that Vale. All the peninſula in Carnarvonſhire called Llun, ſurrounded by St. George's Channel, was ſhook very much. There have been two ſhocks ſince this I have been deſcribing. Mr. PENNANT felt one; but I was not ſenſible of either. The times it was felt at differ very much on account of the variations in the ſeveral dials from whence the clocks are regulated; but I am very exact as to my own time, having the day preceding the earthquake, and that very day, aſcertained my time by equal altitudes, taken with one of Mr. BIRD's aſtronomical quadrants of one foot radius. As every phenomenon of this kind is intereſting, you may, perhaps, wiſh to communicate this account to the Royal Society; which you are welcome to do, if you think it worth the attention of ſo illuſtrious a body.

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* This houſe is built upon the ſide of a rock; and my bed-chamber, though up two pair of ſtairs, is on a ground-floor, the floor is not more than one foot from the ſolid rock in my bed-chamber.

I have the honour to be, &c.

XVIII. *On the Heat of the Water in the Gulf-ſtream.* By Charles Blagden, *M. D. Phyſician to the Army, F. R. S.*

Read April 26, 1781.

ONE of the moſt remarkable facts obſerved in navigating the ocean, is that conſtant and rapid current which ſits along the coaſt of North America to the northward and eaſtward, and is commonly known to ſeamen by the name of the Gulf-ſtream. It ſeems juſtly attributed to the effect of the trade-winds, which blowing from the eaſtern quarter into the great Gulf of Mexico, cauſe there an accumulation of the water above the common level of the ſea; in conſequence of which, it is conſtantly running out by the channel where it finds leaſt reſiſtance, that is, through the Gulf of Florida, with ſuch force as to continue a diſtinct ſtream to a very great diſtance. Since all ſhips going from Europe to any of the ſouthern provinces of North America muſt croſs this current, and are materially affected by it in their courſe, every circumſtance of its motion becomes an object highly intereſting to the ſeaman, as well as of great curioſity to the philoſopher. An obſervation which occurred to me on the ſpot ſuggeſts a new method of inveſtigating a matter that appears ſo worthy of attention.

During a voyage to America in the ſpring of the year 1776, I uſed frequently to examine the heat of ſea-water newly drawn, in order to compare it with that of the air. We made our paſſage far to the ſouthward. In this ſituation, the greateſt heat of the water which I obſerved was ſuch as raiſed the quickſilver in FAHRENHEIT's thermometer to 77°½. This happened twice; the firſt time on the 10th of April, in latitude 21° 10′ N. and longitude, by our reckoning, 52° W; and the ſecond time, three days afterwards, in latitude 22° 7′ and longitude 55°; but in general the heat of the ſea near the tropic of Cancer about the middle of April was from 76° to 77°.

The rendezvous appointed for the fleet being off Cape Fear, our courſe, on approaching the American coaſt, became north-weſtward. On the 23d * of April the heat of the ſea was 74°, our latitude at noon 28° 7′ N. Next day the heat was only 71°; we were then in latitude 29° 12′; the heat of the water, therefore, was now leſſening very faſt in proportion to the change of latitude. The 25th our latitude was 31° 3′; but though we had thus gone almoſt 2° farther to the northward, the heat of the ſea was this day rather increaſed, it being 72° in the morning, and 72°½ in the evening. Next day, the 26th of April, at half after eight in the morning, I again plunged the thermometer into ſea-water, and was greatly ſurpriſed to ſee the quickſilver riſe to 78°, higher than I had ever obſerved it, even within the tropic. As the difference was too great to be imputed to any accidental variation, I immediately conceived that we muſt have come into the Gulf-ſtream, the water of which ſtill retained great part of the heat that it had acquired in the torrid zone. This idea was confirmed by the ſubſequent regular and quick diminution of the heat: the ſhip's run for a quarter of an hour had leſſened it 2°; the thermometer,

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* From the difference between civil and nautical time, it becomes neceſſary to obſerve, that the former is always meant in this paper.

at three quarters after eight, being raiſed by ſea-water freſh drawn only to 76°; by nine the heat was reduced to 73°, and in a quarter of an hour more, to 71° nearly: all this time the wind blew freſh, and we were going ſeven knots an hour on a north-weſtern courſe. The water now began to loſe the fine tranſparent blue colour of the Ocean, and to aſſume ſomething of a greeniſh olive tinge, a well-known indication of ſoundings. Accordingly, between four and five in the afternoon ground was ſtruck with the lead at the depth of eighty fathom, the heat of the ſea being then reduced to 69°. In the courſe of the following night and next day, as we came into ſhallower water and nearer the land, the temperature of the ſea gradually ſunk to 65°, which was nearly that of the air at the time.

Unfortunately bad weather on the 26th prevented us from taking an obſervation of the ſun; but on the 27th, though it was then cloudy at noon, we calculated the latitude from two altitudes, and found it to be 33° 26′ N. The difference of this latitude from that which we had obſerved on the 25th, being 2° 23′, was ſo much greater than could be deduced from the ſhip's run marked in the log-book, as to convince the ſeamen that we had been ſet many miles to the northward by the current.

On the 25th at noon, the longitude by our reckoning was 74° W., and I believe the computation to have been pretty juſt; but the ſoundings, together with the latitude, will determine the ſpot where theſe obſervations were made better than any reckoning from the eaſtward. The ſhip's run on the 26th, from nine in the forenoon to four in the afternoon, was about ten leagues on a north-weſt by north courſe; ſoon afterwards we hove-to in order to ſound, and, finding bottom, we went very ſlowly all night, and till noon the next day.

From theſe obſervations, I think, it may be concluded, that the Gulf-ſtream, about the 33d degree of north latitude, and the 76th degree of longitude weſt of Greenwich, is, in the month of April, at leaſt ſix degrees hotter than the water of the ſea through which it runs. As the heat of the ſea-water evidently began to increaſe in the evening of the 25th, and as the obſervations ſhew that we were getting out of the current when I firſt tried the heat in the morning of the 26th; it is moſt probable, that the ſhip's run during the night is nearly the breadth of the ſtream meaſured obliquely acroſs; that, as it blew a freſh breeze, could not be much leſs than twenty-five leagues in fifteen hours, the diſtance of time between the two obſervations of the heat, and hence the breadth of the ſtream may be eſtimated at twenty leagues. The breadth of the Gulf of Florida, which evidently bounds the ſtream at its origin, appears by the charts to be two or three miles leſs than this, excluding the rocks and ſand-banks which ſurround the Bahama Iſlands, and the ſhallow water that extends to a conſiderable diſtance from the Coaſt of Florida; and the correſpondence of theſe meaſures is very remarkable, ſince the ſtream, from well-known principles of hydraulics, muſt gradually become wider as it gets to a greater diſtance from the channel by which it iſſues.

If the heat of the Gulf of Mexico was known, many curious calculations might be formed by comparing it with that of the current. The mean heat of Spaniſh-town and Kingſton in Jamaica ſeems not to exceed 81°[*]; that of St. Domingo on the ſea-coaſt may be eſtimated at the ſame from Monſ. GODIN's obſervations[*]; but as the coaſt of the continent which bounds the gulf to the weſtward and ſouthward is probably warmer, perhaps a degree or two may be allowed for the mean temperature of the climate over the whole bay: let it be ſtated at 82° or 83°. Now there ſeems to be great probability in the ſuppoſition that the ſea, at a certain comparatively ſmall diſtance below its ſurface, agrees in heat pretty nearly with the average temperature of the air during the whole year in that part; and hence it may be conjectured, that the general heat of the water, as it iſſues out of the bay to form the ſtream, is about 82°[†], the ſmall variations of temperature on the ſurface not being ſufficient to affect materially that of the general maſs. At the tropic of Cancer I found the heat to be 77°; the ſtream, therefore, in its whole courſe from the gulf of Florida, may be ſuppoſed to have been conſtantly running through water from 4° to 6° colder than itſelf, and yet it had loſt only 4° of heat, though the ſurrounding water where I obſerved it was 10° below the ſuppoſed original temperature of the water which forms the current. From this ſmall diminution of the heat, in a diſtance, probably of 300 miles, ſome idea may be acquired of the vaſt body of fluid which ſets out of the gulf of Mexico, and of the great velocity of its motion. Numerous obſervations on the temperature of this ſtream, in every part of it, and at different ſeaſons of the year, compared with the heat of the water in the ſurrounding ſeas, both within and without the tropic, would, I apprehend, be the beſt means of aſcertaining its nature, and determining every material circumſtance of its movement, eſpecially if the effect of the current in puſhing ſhips to the northward is carefully attended to, at the ſame time with the obſervations upon its heat.

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[*] Hiſtory of Jamaica, London, 1774, vol. III. p. 652, 653. The different obſervations of the heat recorded in that work do not agree together; but thoſe adopted here are taken from that ſeries which appeared to me the moſt correct.

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[*] Monſ. GODIN's experiments upon the pendulum were made at the Petit Goave. They continued from the 24th of Auguſt to the 4th of September, and the average heat during that time was ſuch as is indicated by 25° of Monſ. DE REAUMUR's thermometer (ſee Mem. Acad. Scienc. 1735, p. 517.). According to Monſ. DE LUC's calculation (ſee Modifications de l'Atmoſpere, vol. I. p. 378.) the 25th degree of Monſ. DE REAUMUR's *true* thermometer anſwers to about the 85th of FAHRENHEIT's; but the average heat in Jamaica during the months of Auguſt and September is alſo 85°; hence we may conclude, that the mean heat for the whole year is nearly the ſame on the ſea-coaſts in both iſlands.

[†] The loweſt calculation of the mean temperature of the gulf is preferred on this occaſion, becauſe of the conſtant influx of new water from the Atlantic Ocean produced by the trade-winds; which water not having been near any land muſt, I think, be ſenſibly cooler than that which has remained ſome time incloſed in the bay. On this ſubject the obſervations made by ALEXANDER DALRYMPLE, Eſq. relative to the heat of the ſea near the Coaſt of Guinea, ought to be conſulted (ſee Phil. Tranſ. vol. LXVIII. p. 394, &c.).

On the 25th of September 1777, as the ſhips which had tranſported Sir WILLIAM HOWE's army up Cheſapeak Bay were returning toward the Delaware, with the ſick and ſtores, they were overtaken, between Cape Charles and Cape Hinlopen, by a violent gale of wind; which, after ſome variation, fixed ultimately at N.N.E. and continued five days without intermiſſion. It blew ſo hard that we were conſtantly loſing ground, and driving to the ſouthward; we alſo purpoſely made ſome *eaſting* to keep clear of the dangerous ſhoals which lie off Cape Hatteras.

The 28th at noon our latitude was 36° 40′ N. and the heat of the ſea all day about 65°. On the 29th our latitude was 36° 2′; we had, therefore, in the courſe of theſe twenty-four hours, been driven by the wind 38 nautical miles to the ſouthward:

ward: the temperature of the ſea continued nearly at 65°. Next day, the 30th, our latitude at noon was 35° 44′, only 18 miles farther to the ſouthward, though in the opinion of the ſeamen aboard, as well as my own, it had blown at leaſt as hard on this as any of the preceding days, and we had not been able to carry more ſail; conſequently it may be concluded, that ſome current had ſet the ſhip 20 miles to the northward. To know whether this was the Gulf-ſtream, let us conſult the thermometer. At half after nine in the forenoon of this day the heat of this water was 76°, no leſs than *eleven* degrees above the temperature of the ſea before we came into the current!

Towards evening the wind fell, and we ſtood N.W. by N. cloſe-hauled. As the ſea ſtill ran very high, and the ſhip ſcarcely went above two knots an hour, we did not make leſs than three points of lee-way on this tack; the courſe we made good, therefore was W.N.W. which, on the diſtance run by noon next day, gave us about ſixteen miles of *northing*; but that day, the 1ſt of October, our latitude was 36° 22′, 38 miles farther to the north than we had been the day before; the difference, 22 miles, muſt be attributed to the Gulf-ſtream. This, however, is only part of the effect which the current would have produced upon the ſhip if we had continued in it the whole four and twenty hours; for, though we were ſtill in the ſtream at five in the afternoon of the 30th, as appeared by the heat of the water being then above 75°, and at eight in the evening the heat being ſtill 74°, yet by ſeven next morning we were certainly got clear of it, the heat of the ſea being then reduced to its former ſtandard of 65°. On this occaſion, therefore, we did not croſs the ſtream, but having fallen-in with it obliquely on the weſtern ſide, we puſhed out again on the ſame ſide as ſoon as the gale abated.

Theſe obſervations having been made three degrees to the northward of my former ones, it is curious to obſerve, that the heat of the Gulf-ſtream was about 2° leſs. The ſeaſons of the year, indeed, were very different; but, perhaps, under ſuch circumſtances that their effects were nearly balanced. In the latter obſervations the meridian altitude of the ſun was leſs; but then a hot ſummer preceded them: whereas in the former, though the ſun's power was become very great, yet the winter had been paſt but a ſhort time. Calculating upon this proportion we may be led to ſuſpect, that about the 27th degree of latitude, which is as ſoon as the ſtream has got clear of the gulf of Florida, it begins ſenſibly to loſe its heat from 82°, the ſuppoſed temperature of the gulf of Mexico, and continues to loſe it at the rate of about 2° of FAHRENHEIT's ſcale to every 3° of latitude, with ſome variation, probably as the ſurrounding ſea, and the air, are warmer or colder at different ſeaſons of the year.

The preceding facts had made me very deſirous of obſerving the heat of the Gulf-ſtream on my paſſage homeward; but a violent gale of wind, which came on two days after we had ſailed from Sandy Hook, diſabled every perſon aboard, who knew how to handle a thermometer, from keeping the deck. The maſter of the ſhip, however, an intelligent man, to whom I had communicated my views, aſſured me, that on the ſecond day of the gale the water felt to him remarkably warm; we were then near the 70th degree of weſt longitude. This agrees very well with the common remark of ſeamen, who alledge, that they are frequently ſenſible of the Gulf-ſtream off Nantucket ſhoals, a diſtance of more than 1000 miles from the gulf of Florida! According to the calculation I have before adopted of a loſs of two degrees of heat for every 3° of latitude, the temperature of the Gulf-ſtream here would be nearly 73°; the difference of which from 59°, the heat that I obſerved in the ſea-water both before and after the gale, might eaſily be perceived by the maſter of the veſſel. This was in the winter ſeaſon, at the end of December.

An opinion prevails among ſeamen, that there is ſomething peculiar in the weather about the Gulf-ſtream. As far as I could judge, the heat of the air was conſiderably increaſed by it, as might be expected; but whether to a degree or extent ſufficient for producing any material changes in the atmoſphere muſt be determined by future obſervations.

Perhaps other currents may be found which, iſſuing from places warmer or colder than the ſurrounding ſea, differ from it in their temperature ſo much as to be diſcovered by the thermometer. Should there be many ſuch, this inſtrument will come to be ranked among the moſt valuable at ſea; as the difficulty of aſcertaining currents is well known to be one of the greateſt defects in the preſent art of navigation.

In the mean time, I hope the obſervations which have been here related are ſufficient to prove, that in croſſing the Gulf-ſtream very eſſential advantages may be derived from the uſe of the thermometer: for if the maſter of a ſhip, bound to any of the ſouthern provinces of North America, will be careful to try the heat of the ſea frequently, he muſt diſcover very accurately his entrance into the Gulf-ſtream, by the ſudden increaſe of the heat; and a continuance of the ſame experiments will ſhew him, with equal exactneſs, how long he remains in it. Hence he will always be able to make a proper allowance for the number of miles that the ſhip is ſet to the northward, by multiplying the time into the velocity of the current. Though this velocity is hitherto very imperfectly known, for want of ſome method of determining how long the current acted upon the ſhips, yet all uncertainty ariſing from thence muſt ſoon ceaſe, as a few experiments upon the heat of the ſtream, compared with the ſhip's run checked by obſervations of the latitude, will aſcertain its motion with ſufficient preciſion. From differences in the wind, and perhaps other circumſtances, it is probable, that there may be ſome variations in the velocity of the current; and it will be curious to obſerve, whether theſe variations may not frequently be pointed out by a difference in its temperature; as the quicker the current moves, the leſs heat is likely to be loſt, and conſequently the hotter will the water be. In this obſervation, however, the ſeaſon of the year muſt always be conſidered; partly, becauſe it may, perhaps, in ſome degree affect the original temperature of the water in the gulf of Mexico; but principally, becauſe the actual heat of the ſtream muſt be greater or leſs in proportion as the tract of the ſea through which it has flown was warmer or colder. In winter, I ſhould ſuppoſe, that the heat of the ſtream itſelf would be rather leſs than in ſummer; but that the difference between it and the ſurrounding ſea would be much greater; and I can conceive that, in the middle of ſummer, though the ſtream had loſt very little of its original heat, yet the ſea might, in ſome parts, acquire ſo nearly the ſame temperature, as to render it ſcarcely poſſible to diſtinguiſh by the thermometer when a ſhip entered into the current.

Beſides the convenience of correcting a ſhip's courſe, by knowing how to make a proper allowance for the diſtance ſhe is ſet to the northward by the current, a method of determining with certainty when ſhe enters into the Gulf-ſtream is attended with the further ineſtimable advantage of ſhewing her place upon the ocean in the moſt critical ſituation: for, as the current ſets along the coaſt of America at no great diſtance from ſoundings, the mariner, when he finds this ſudden increaſe of heat in the ſea, will be warned of his approach to the coaſt, and will thus have timely notice to take the neceſſary precautions for the ſecurity of his veſſel. As the courſe of the Gulf-ſtream comes more to be accurately known, from repeated obſervations of the heat and latitudes, this method of determining the ſhip's place will be proportionably more applicable to uſe. And it derives additional importance from the peculiar circumſtances of the American coaſt, which, from the mouth of the Delaware to the ſouthernmoſt point of Florida, is every where low, and beſet with frequent ſhoals, running out ſo far into the ſea that a veſſel may be aground in many places where the ſhore is not to be diſtinguiſhed even from the maſt-head. The gulf-ſtream, therefore, which has hitherto ſerved only to increaſe the perplexities of ſeamen, will now, if theſe obſervations are found to be juſt in practice, become one of the chief means of their preſervation upon that dangerous coaſt.

XIX. *Account of the Appearance of the Soil at opening a Well at* Hanby *in* Lincolnſhire. *In a Letter from Sir* Henry C. Englefield, *Bart. F. R. and A. S. to Sir* Joſeph Banks, *Bart. P. R. S.*

Read May 3, 1781.

DEAR SIR,

THE appearance of the ſoil which fell under my own inſpection, on opening a well at Hanby, the ſeat of Sir C. BUCK, in Lenton pariſh, Lincolnſhire, being, as far as I can recollect, quite ſingular, I hope you will not think this account of it unworthy the attention of the Society.

The ſpot on which the well was ſunk is nearly on a level with Lincoln Heath, and of courſe high ground compared with the fen, which is diſtant from it above ſix miles. The ſoil was uniformly a blue clay, in parts rather inclining to a ſhaly ſtructure, and contained many caſts of tellinæ, a very little pyrites, and ſome few ſmall, but very elegant, belemnites. Theſe are all the uſual foſſils of clay; but what I think without example is, that through the whole maſs of clay were interſperſed nodules of pure chalk, evidently rounded by long attrition, and of all ſizes from that of a pea to a child's head.

They lay in no ſort of order that I could find. How deep this appearance might have continued I cannot determine, but no water having been found at the depth of thirty feet, the trial was given up, as the expence would have exceeded the advantage propoſed. A ſpecimen of the chalk is herewith exhibited to the Society.

I muſt add, that in all the environs there is not the leaſt trace of chalk in any form whatever that I could diſcover or hear of.

I am, &c.

XX. *Aſtronomical Obſervations made by* Nathaniel Pigott, *Eſq. F. R. S. Foreign Member of the Academies of* Bruſſels *and* Caen, *and Correſpondent of the Academy of Sciences at* Paris. *Communicated by Sir* Henry C. Englefield, *Bart. F. R. and A. S.*

Read May 3, 1781:

HAVING given in the Philoſophical Tranſactions, vol. LXVI. for the year 1776, and vol. LXVIII. part II. for 1778, an ample deſcription of the excellent aſtronomical inſtruments in my poſſeſſion, it will be needleſs to ſay any thing here on that ſubject, further than that the following obſervations were made with them.

During the ſummer of 1777, part of which I ſpent with Lady WIDDRINGTON, at her houſe named Wickhill, about a mile from Stow on the Would, Glouceſterſhire, I determined, by ſix obſervations of ♃'s ſatellites, compared to correſpondent ones made on the ſame days, Wickhill W. of Greenwich 1° 29′ 45″. It is proper to add, this is likewiſe the longitude of Stow, it being under the ſame meridian, or very nearly ſo, as Wickhill.

In 1778 and 1779 I obſerved in Glamorganſhire; and by thirty-five meridian obſervations of the ſun and ſtars, all agreeing within 12″ from the mean, I determined the latitude of my obſervatory at Frampton Houſe 51° 25′ 1″ N.

Frampton-houſe lies between Cowbridge and Lantwit; about four miles ſouth of the former, and one mile north of the latter, and about two miles from the Briſtol channel; is nearly under the ſame meridian as Watchet, a market town in Somerſetſhire.

The rocks on the Welch coaſt, which run obliquely ſlanting into the Briſtol channel, render the navigation ſo dangerous, that each year affords the horrid ſpectacle of ſhips wrecked; and here I am ſorry to add, that the barbarous cuſtom of plundering theſe unfortunate veſſels ſtill ſubſiſts in all its inhumanity; at the ſame time it would be injuſtice to the gentlemen of the country to paſs under ſilence their repeated endeavours to check this enormity; but hitherto their efforts have not been attended with much ſucceſs: it is due to humanity to make ſuch bad practices public, in hopes of exciting an inquiry, which juſtice and the honour of the nation loudly call for.

The little that has been ſaid ſuffices to ſhew the expediency of correct maps of this channel. The univerſal opinion of the country is, that to the Somerſetſhire oppoſite coaſt, about Watchet, Purlock, &c. the breadth of the channel is twenty or twenty-one miles. A ſingle glance of the eye ſeems ſufficient to contradict this notion; however, as upon inſpecting my maps I found the diſtances ſet down not greatly different from what report had made them, we meaſured them geometrically, and the reſult gave the channel, not twenty, but little more than thirteen miles broad at the abovementioned places.

Upon the whole it is to be wiſhed, that aſtronomical obſervations, ſufficiently correct, were made on the Somerſetſhire ſide, which might be compared with thoſe I have made on the oppoſite ſhore. It may poſſibly be found, that the towns on the

Engliſh coaſt are placed in the maps too much ſouth, and thoſe in Wales too much north; and hence, perhaps, the too great breadth given to the Briſtol channel. This is, at leaſt, the caſe with the town of Lantwit, which, as I have ſaid, is to the ſouthward of Frampton-houſe, the latitude of which is 51° 25′ 1″; nevertheleſs the beſt and moſt extenſive map * I have been able to procure gives the latitude of Lantwit 51° 29′ 40″, that is to ſay 4′ 39″ N. of Frampton-houſe. There may very poſſibly be particular charts of the Briſtol channel more exact; but it is not leſs true, that the common maps ought to be cleared of ſuch enormous errors.

I determined the difference of meridians between Frampton-houſe and Greenwich by comparing four immerſions and fourteen emerſions of ♃'s firſt and ſecond ſatellites to correſponding ones made in other obſervatories. The reſult is as follows:

App. time. ′	″	Immerſions.
14	11	By an obſerv. at Greenwich.
13	52	———— at Upſal.
13	54	———— at Paris.
14	6	———— at Oxford.
14	1	the mean.

App. time. ′	″	Emerſions.
13	43	By an obſerv. at Oxford.
14	2	———— at Greenwich.
13	50	———— N. Almanack.
13	45	———— at Greenwich.
13	46	———— at Oxford.
13	44	———— at Paris.
14	12	———— at Paris.
13	50	———— at Oxford.
14	11	———— at Berlin.
13	42	———— at Oxford.
13	50	———— at Greenwich,
13	59	———— at Oxford.
14	3	———— at Greenwich.
14	7	———— at Greenwich.
13	55	Mean of emerſions.
14	1	Mean of immerſions.
13	58	by a mean of the means

Frampton-houſe W. of Greenwich in time, or 3° 29′ 30″ by the equator.

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* By JOHN ADAMS. The ſcale is to minutes of a degree.

Occultations of fixed ſtars obſerved at Frampton-houſe in 1777 and 1778.

App. time. October 21, 1777.

18° 16′ 20″ Imm. ϑ Pollux; doubtful to 2″ or 3″.

November 15.

6 19 3 Imm. 1ſt, ϑ Tauri into light part of ☽: doubtful to 3″.
6 54 22 Imm. 2d, ϑ Tauri.
7 8 42 Emerſ. 1ſt, ϑ Tauri: dark part of ☽.

November 16.

10 59 27 Imm. ζ Tauri: very good.

July 5, 1778.

9 2 15½ Imm. teleſcopic ſtar: inſtantaneous.
9 3 28½ Imm. *vm*: inſtantaneous.
10 27 8½ Emerſ. *vm*, from light limb of ☽: ſure to 1″ or 2″.

Declination of the needle.

In the beginning of 1778 the declination weſt of a magnetic needle of four inches made by Mr. DOLLOND, appeared to be 22° 11′.

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II. *Abstract of a Register of the Barometer, Thermometer, and Rain, at* Lyndon, *in* Rutland, 1780. *By* Thomas Barker, *Esquire.*

Read May 3, 1781.

	Barometer.			Thermometer.						Rain.	Mean rain.	
				In the Houſe.			Abroad.				10 yrs. 71—80	44 yrs. 45 yrs. 36—80
	Higheſt.	Loweſt.	Mean.	High.	Low.	Mean	High.	Low.	Mean			
Morn. Aftern.	29,89	28,16	29,39	39 40	28 29	34 35	36½ 43	15½ 22½	27 32	1,013	1,677	1,573
Morn. Aftern.	30,06	28,72	29,49	44 44½	33 34	37 38	41 46	22 30½	31 39	1,572	1,871	1,378
Morn. Aftern.	29,94	28,99	29,47	51½ 53½	41½ 43½	46 47½	49 61	30 45	40 51	1,175	1,355	1,315
Morn. Aftern.	29,77	28,40	29,21	53 55	40 41½	46 46	52 60	29½ 38	38 48	2,727	1,314	1,465
Morn. Aftern.	29,83	28,80	29,48	67 72	51 52½	55 57	65 80½	40½ 53	50½ 61	1,201	2,081	1,610
Morn. Aftern.	29,89	29,26	29,55	65½ 71½	52 52½	57½ 59	62½ 80	43 51	53½ 64	1,920	2,374	2,249
Morn. Aftern.	29,87	29,21	29,61	69½ 71½	58½ 59½	63 64	65 83	51½ 61	57½ 69½	1,566	2,507	2,516
Morn. Aftern.	29,81	29,43	29,65	68 72	61½ 63	64 66	63 81½	52 63	58 70	0,432	2,468	2,247
Morn. Aftern.	29,79	28,70	29,38	70 73½	53 53½	61 63	61 82	43 53½	52 65	3,427	3,142	2,016
Morn. Aftern.	29,87	28,20	29,23	58½ 59	46½ 48	51 52	56 64	33½ 45½	44 53	3,080	2,975	2,158
orn. ftern.	30,00	28,62	29,41	50 50½	38½ 38½	43 43½	48½ 48	19½ 31	36 40	1,461	2,372	1,943
orn. ftern.	30,08	29,22	29,83	45 45½	35 35	40 40½	46½ 50	26 30½	35 38½	0,534	1,889	1,740
										20,108	26,024	22.210

The three drieſt ſeaſons from one month to twelve are the ſame as in vol. LXI. except the following.

Months						
2			Feb. and Mar. 79	0,370	Jan. and Feb. 79	0,451
3	Jan.—Mar. 79	0,582	Dec. 42—Feb. 43	0,934	Jan.—Mar. 40	0,942
7					Oct. 39—Apr. 40	0,543
10			Oct. 40—July 41	10,174		
12	Sept. 40—Aug. 41	13,427				

The three wetteſt ſeaſons from one month to twelve are entirely different from thoſe in vol. LXI.

Months.						
1	Sept. 74.	8,000	Nov. 70	7,818	May 73	6,843
2	Aug. and Sept. 74	11,910	Oct. and Nov. 70	10,932	Aug. and Sept. 75	10,430
3	July—Sept. 74	15,137	July—Sept. 75	14,508	Oct.—Dec. 70	13,545
4	July—Oct. 75	17,988	June—Sept. 74	17,620	Oct. 70—Jan. 71	14,958
5	July—Nov. 75	21,558	May—Sept. 74	20,762	Aug.—Dec. 70	17,038
6	July—Dec. 75	22,654	April—Sept. 74	22,285	Jun.—Nov. 70	18,978
7	July 75—Jan. 76	25,165	Mar.—Sept. 74	25,013	July 63—Jan. 64	22,902
8	July 75—Feb. 76	28,360	Feb.—Sept. 74	26,959	May—Dec. 73	25,623
9	Jan.—Sept. 74	30,267	July 75—Mar. 76	29,878	May 73—Jan. 74	28,931
10	Dec. 73—Sept. 74	33,164	May 73—Feb. 74	30,877	July 75—April 76	30,765
11	Nov. 73—Sept. 74	36,769	May 73—Mar. 74	33,605	July 75—May 76	32,392
12	Oct. 73—Sept. 74	39,390	May 73—April 74	35,128	May 75—April 76	34,877

The year began with froſt, and was perhaps the ſevereſt winter ſince 1740, but there was not a great deal of ſnow, and in general it was calm. The froſt was not ſo ſteady as it was that winter, there being ſeveral breaks in it; but was very ſharp, and the ice was never intirely gone for nine or ten weeks together from December 22, till near the end of February, when it went away without wet, leaving the ground remarkably light and fine, and the weather grew mild, and continued ſo moſt part of March; but the coldneſs of the ground hindered the graſs from growing greatly till toward the end of the time. The feed-time was fine and good, and the grain came up very well, but the firſt three weeks of April were cold, backening, and often froſty. Toward the end it was more ſhowery, warmer and growing, and from that time the ſpring continued to come on, and there were ſo few N.E. winds that ſhips found a difficulty in getting down the channel, which is very unuſual at that time of year, and all the ſpring from the end of February till toward the end of June was very windy, chiefly N.W. and S.W.

In the former part of ſummer there were at times very hot days; but the ſeaſon was oftener cool; many little ſhowers, which in ſome countries were ſo ſmall there was want of rain and graſs, here we did pretty well. The hay-time was fine, but the crop ſmall. The harveſt was exceedingly well got: the barley and oats good, and ſome of the wheat; but the late-ſown was thin through the ſevere winter, and in ſeveral places the wheat was mildewed, which could not be by wet ſuch a year as this; but by this means wheat became three times the price of barley, being 50 and 52 ſhillings a quarter, and barley 16 or 17 ſhillings. From the latter part of July to the beginning of September it was very dry, hot, and burning; much ſcorching ſun, the ground very much burnt up, and great want of water; but the N.E. winds, which came at this time of year inſtead of the ſpring, were ſometimes freſh and cool.

The beginning of September the rains began, and for above two months there was a good deal, with ſuch fine and warm weather, that there was good graſs, a pleaſant autumn, and very few froſty mornings, and the ground, which before was ſo dry, did not get much dirty with it. The wheat ſeed-time was fine, and the weather mild till the middle of November, when a hard froſt, with ſnow, made people think of a hard winter; but it grew mild again, was chiefly dark and cloudy, but little rain, and drying a good deal of December; remarkably calm, but near a week's froſt about Chriſtmas.

The ſickly ſeaſons, which began in Auguſt 1779, continued more or leſs all the year; and about the ſame time of the year increaſed again. There were great numbers of fevers and agues, eſpecially in and near the fens, which were very obſtinate, and did not yield to the uſual medicines, but frequently returned again, and hung very long on the patients.

XXII. *Some Calculations of the Number of Accidents or Deaths which happen in conſequence of Parturition; and of the Proportion of Male to Female Children, as well as of Twins, monſtrous Productions, and Children that are dead-born; taken from the Midwifery Reports of the* Weſtminſter General Diſpenſary: *with an Attempt to aſcertain the Chance of Life at different Periods, from Infancy to Twenty-ſix Years of Age; and likewiſe the Proportion of Natives to the reſt of the Inhabitants of* London. *In a Letter from* Robert Bland, *M. D. Phyſician-Man-Midwife to the* Weſtminſter General Diſpenſary, *to* Samuel Foart Simmons, *M. D. F. R. S.*

Read May 10, 1781.

DEAR SIR,

THE great advantage of hoſpitals and other ſimilar inſtitutions, in improving and diſſeminating medical knowledge, is generally acknowledged; but there are other purpoſes they ſeem equally calculated to anſwer, which, though ſubordinate to the former, may yet deſerve attention, as they may throw light upon, and perhaps finally determine, certain political queſtions, about which various opinions are at preſent entertained. Thus, though it is known that this city contains perſons from various countries, and that a very ſmall portion of its inhabitants are natives; yet the proportion which the latter bear to the aliens can at preſent only be gueſſed at.

But this queſtion might be reſolved, with a tolerable degree of accuracy, if to the regiſter of the names of the perſons admitted to the ſeveral charities were added the places of their birth. Again, the great mortality of the human ſpecies, particularly in infancy, and the ſmall chance a child has to attain to years of maturity, have been calculated from pariſh-regiſters, bills of mortality, &c. But I do not know, that an attempt has hitherto been made to aſcertain them, by noting the number of children a promiſcuous multitude of women had borne, together with the number they had been able to preſerve.

This, however, is what I have here done; and from it I have attempted to form a table, ſhewing the chance of life at different periods. I am far from pretending that by this mode clear and certain intelligence will be obtained; but in a matter of ſuch moment, I preſume, that any aſſiſtance will be acceptable. Dr. SMELLIE * has curſorily mentioned, for the encouragement of his pupils, the ſmall proportion of the unnatural and laborious births to the natural; but he did not carry his views farther, or point out the proportionate number of conſequent accidents, which might occur to retard or prevent the recovery of the woman, although this is not leſs neceſſary to be known than the former. With a view to theſe, and other uſeful purpoſes, the following regiſter has been kept of the moſt material circumſtances concerning the patients admitted to the midwifery-department of the Weſtminſter General Diſpenſary, from its firſt inſtitution, in the year 1774, to the preſent time; *viz.*

1. The ages of the ſeveral women.
2. The number of children they had borne.
3. The ſexes of the children,

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* See SMELLIE's Midwifry, 8vo, p. 195.

4. The number of children they had been able to preſerve.

5. The place or country where they and their huſbands were born.

And after the delivery of the patient I have conſtantly noted,

1. The accidents that attended, or were the conſequences of parturition.

2. The ſexes of the children delivered.

3. The number of twins or triplets.

4. The number of the children that were deficient or monſtrous.

5. The number of the children that were dead-born: and, as the women were enjoined to return their letters as ſoon as they were able to go abroad, I farther intended to have added the proportion of the children who died under four or five weeks; but many of the women neglecting this duty, prevented my information under this head from being ſo compleat as I could have wiſhed. Of thoſe, however, who came, or of whom certain account could be obtained, the number is ſet down.

From the above mentioned regiſter the following tables and accounts have been compoſed; and as the greateſt care and exactneſs were uſed in recording the ſeveral circumſtances, the ſame punctuality has been obſerved in collecting and digeſting them. And that they might be kept as free from error as poſſible, tables for each year were firſt compoſed and compared together; but finding no material variation, I did not think it neceſſary to produce them in that form. My firſt intention was to have given the tables ſimply, and without any explanatory obſervations; but finding I could not introduce all the circumſtances I had noted in my regiſter, as was particularly the caſe with regard to the firſt table, and imagining that in ſome places they were not perfectly intelligible, without ſome explanation, as for inſtance in the table of the chance of life at different periods, I have ventured to add ſuch occaſional remarks as I think will tend to illuſtrate the ſubject.

As my firſt view was to find the proportion of difficult labours, and of the accidents or deaths that happen in conſequence of child-birth, I ſhall begin with the following TABLE.

Of 1897 women delivered under the care of the Diſpenſary,
63 or 1 in 30 had unnatural labours: in
  18 of theſe, or 1 in 105, the children preſented by their feet; in
  36 or 1 in 52, the breech preſented; in
  8 the arms preſented; and in } 9*, or 1 in 210.
  1 the funis. }
  63

17 women, or 1 in 111, had laborious labours: in
  †8 of theſe, or 1 in 236, the heads of the children were leſſened; in
  4 a ſingle blade of a forceps was uſed; and in the remaining
  5 in which the faces of the children were turned to the pubes, the delivery was at length accompliſhed by the pains.
80 17

80

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* In all theſe nine caſes the children were turned.

† Two of theſe women have ſince been delivered of full-ſized healthy children. A third bore a very ſmall and weakly child, who died in two or three days. A fourth

80

1 woman had convulſions about the ſeventh month of her pregnancy, and was delivered a month after of a dead child, and recovered.

1 woman had convulſions during labour; brought forth a live child, and recovered.

*9 women, or 1 in 210, had uterine hæmorrhage before and during labour.

Of theſe 1 died undelivered;
1 died a few hours, and
1 ten days, after delivery, and
6 recovered.
—
9

5 women had the puerperal fever, of whom four died. In one of theſe the placenta was undelivered, and continued ſo to her death.

2 women were ſeized with mania, but recovered in about three months. In

1 woman a ſuppuration took place, ſoon after labour, from the vagina into the bladder and rectum. This patient recovered, but the urine and ſtools continue to paſs through the wounds. Of

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99

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fourth was delivered of a ſeven-months child, without mutilating it, which died in its paſſage. The number of women, therefore, who from error in their conformation were incapable of bearing live children appears to be very inconſiderable. Of the remaining four I have not been able to get any intelligence.

* In theſe nine caſes only one child was ſaved.

99
1 woman the perinæum was lacerated to the ſphincter ani. A ſuture was attempted, but without effect; ſhe recovered, but is troubled with prolapſus uteri.
5 had large and painful ſwellings of the legs and thighs, but recovered.

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105 therefore of theſe, or 1 in 18, had preternatural or laborious births, or ſuffered in conſequence of labour. Of this number of caſes 43, or 1 in 44, were attended with particular difficulty or danger; and 7 only, or 1 in 270, died. The remaining 62 were delivered and recovered with little more than the common aſſiſtance: and
1792 had natural labours, not attended with any particular accidents.

1897

Of two women the uterus was retroverted in the third or fourth month of their pregnancy; but in both the uterus was replaced, and the women went to their full time, and brought forth live children.

Beſides the accidents above enumerated, it ſeems right to obſerve, that many of the women were afflicted with ſevere after-pains, or had what is called the milk-fever; but as theſe complaints were generally relieved in three or four days, and did not ſeem to have any influence in retarding their recovery, or to affect their future healths, no notice is taken of them. Some women alſo had ſymptoms of incipient prolapſus uteri, who had not before been troubled with that complaint; but as

I ſeldom

I ſeldom ſaw them above once or twice, I cannot give the reſult. But as few of them were able to indulge themſelves with reſt, or to comply with the rules neceſſary for their cure, it is to be feared, that in many of them the complaint would gradually increaſe; and that in a courſe of years, the uterus would make its appearance externally, when, finding it an impediment to their activity, they would apply and ſubmit to wear a peſſary, or to ſuch palliatives as in that ſtate can only be adminiſtered. Excepting this accident, and the fluor albus, to which many of them are ſubject after child-bearing, but which, I think, does not often materially affect their healths until late in life, I am inclined to believe, that the lower ſort of people recover more certainly after parturition than perſons in higher ſtations of life: at leaſt, they are leſs ſubject to the puerperal fever, which is ſo fatal, if not checked on its firſt attack; and which, if not cauſed, is certainly nouriſhed, and its malignancy increaſed, by great fires, cloſe rooms, warm ſeptic diet and coſtiveneſs. But the apartments of the poor are generally ſo crazy, that without opening doors or windows, to which they are ſufficiently averſe, the air pours in upon them from all ſides. To this circumſtance, added to their inability to keep great fires, or to indulge themſelves with animal food, and to the care that is taken very early to empty their bowels, I have been induced to attribute their ſo generally eſcaping this fatal diſeaſe; and by adopting rules in my private practice conſonant to this idea, I have the ſatisfaction to be able to ſay, that I have not ſeen the puerperal fever among my private patients for more than three years.

TABLE of the proportion of male to female children, of the number of twins, and of the children that were deficient or monſtrous, and of thoſe that were dead-born.

1897 women were delivered of 1923 children; 972 boys and 951 girls, or as 46 boys to 45 girls.

23 of the women, or 1 in 80, were delivered of twins, 16 of whom were boys and 30 girls.

1 woman was delivered of 3 girls.

Of the twins and triplets, therefore, the males were only half the number of the females.

8 of the children, or 1 in 241, were deficient or monſtrous.

Of theſe 1 was web-fingered;
1 had a hare-lip;
1 had a dropſical head and diſtorted ſpine;
1 a dropſical head;
in 1 a part of the palate;
and in 2 a conſiderable portion of the cranium * was wanting;
and 1 had two heads †, ſee fig. 1.
8

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* One of theſe lived an hour after it was born.

† This was the child of ELIZABETH wife of —— BROMFIELD, Peruke-maker, Jees-court, Oxford-ſtreet. It had two heads and necks, four hands and arms, two ſpines, uniting at the ſacrum, and terminating in one pelvis, from whence the lower extremities proceeded ſingle: there was one navel-ſtring, and one male organ of generation. On opening the body there were found, two thoracic cavities, the right more compleat than the left: the heart alſo, and the lungs on the right ſide, were more perfect than thoſe on the left, which latter were very ſmall.

One woman was delivered of a twin[*], ſee fig. 2.

84 of the children, or 1 in 23 of the whole number, were dead-born[†]. Of theſe, 49, or nearly five-eighths, were boys, and 35 were girls.

Of 1400 women who returned their letters, or of whom a certain account could be obtained, 85, or nearly 1 in 16, had

buried

There were two ſtomachs, two ſets of inteſtines, which, at length uniting, terminated in one rectum and anus. There was but one urinary bladder. The drawing that accompanies this will give a more juſt idea of its external figure; and Dr. HUNTER, who diſſected it, will probably one day oblige the world with an exact anatomical deſcription of it.

[*] Of this ſingular production, to which I have not ventured to give a name, the following is the hiſtory and deſcription. The woman who produced it is about twenty-ſeven years of age; this was her firſt pregnancy. She was, after a labour, delivered of a female fœtus, and its placenta, in which nothing uncommon was obſerved; and although the uterus remained of an unuſual ſize, yet the pains not recommencing, there was no ſuſpicion entertained but that its bulk was occaſioned by coagulated blood. On the third day the pains became violent, and this monſter was born. Its ſhape was ſpherical, but ſomewhat flattened. It meaſured in its largeſt diameter eight inches, and weighed about eighteen ounces. It received its nouriſhment by an umbilical chord, to which was attached a portion of membranes, and although no placenta was found, it is probable it had a ſmall one, and that it was incloſed in its own involucrum. It was completely covered with a cuticula, and a little above the part, where the navel-ſtring terminated, there was a hairy ſcalp covering a bony prominence, ſomewhat reſembling the arch of the cranium. On diſſection it was found to be plentifully ſupplied with blood veſſels, proceeding from the navel-ſtring, and branching through every part of it. It had a ſmall brain and medulla ſpinalis continued into a bony theca, with nerves paſſing from thence through the foramina of the bones; but no reſemblance of any thoracic or abdominal viſcera. The reſt of its bulk was made up of fat.

[†] By dead-born children I mean thoſe that die after they have been perceived to move, that is, generally after four months. Abortions, or deaths before that period, may reaſonably be eſtimated at double this number; ſo that, perhaps, 1 child in 8 dies in the womb, or in the act of coming into the world.

buried their children before the end of two months. Of this number 53, or 5 in 8, were boys, and 32 girls.

This ſingular circumſtance of there being a greater number of males than females among the ſtill-born children, and of a greater number of male children dying in infancy than of females, has been remarked by Dr. PRICE and other writers on calculations; and Dr. HAYGARTH * has ſhewn that at Cheſter more huſbands die in a given period than wives. This naturally ſuggeſts an enquiry, whether the lives of males are at all ages more precarious than thoſe of females.

To be enabled to aſſiſt in anſwering this queſtion, I have added the following article to my regiſter, *viz.* of the children that ſhall be living at the time the women apply for their letters, how many will be boys, and how many girls?

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* Obſervations on the bills of mortality in Cheſter for the year 1772.

TABLE of the ages at which women begin and ceaſe to be capable of bearing children, and of the intermediate periods at which they are moſt ſo.

Of 2102 * pregnant women

		Years of age.	
†36	or 1 in 58 were from -	15 to 19	85, or 1 in 25, from 15 to 20 incluſive.
49	or 1 in 43 were -	20	
578	or 5 in 19 were from -	21 to 25	1684, or four-fifths were from 21 to 35 incluſive.
699	nearly 1 in 3 were from	26 to 30	
407	nearly 1 in 5 were from	31 to 35	
291	or 3 in 22 were from -	36 to 40	
36	or 1 in 58 were from -	41 to 45	42, or 1 in 50, from 41 to 49.
6	or 1 in 350 were from	46 to 49	
2102			

* Although 2102 women, the number here mentioned, obtained letters, entitling them to the aſſiſtance of the midwives, 1897 only were delivered by them: the remainder either removed out of the bounds of the Diſpenſary, or, from ſome alteration in their circumſtances, were obliged to go to an hoſpital or workhouſe.

† 1 of theſe women was between 15 and 16 years of age.
1 between 16 and 17;
3 between 17 and 18;
10 between 18 and 19; and
21 between 19 and 20.

36

TABLES of the number of children borne by 1389 * women, with the number that were living at the time of their applying to the Diſpenſary.

Women.	N° of children borne by each woman.	Total of children born.	Total of children living.	N° of women who had preſerved their children.	N° of children preſerved by each woman.	Total of children preſerved.
1	24	24	5	—	—	—
1	17	17	3	—	—	—
3	16	48	5	—	—	—
2	14	28	11	—	—	—
11	13	155	46	—	—	—
14	12	168	44	—	—	—
15	11	165	45	1	11	11
22	10	220	84	—	—	—
33	9	297	93	—	—	—
56	8	448	151	4	8	32
74	7	518	213	3	7	21
89	6	534	214	11	6	66
138	5	690	288	32	5	160
169	4	676	293	84	4	336
208	3	624	299	174	3	522
254	2	508	259	306	2	612
299	1	299	171	464	1	464
1389		†5419	2224	1079		2224
and 370 were in their firſt pregnancy,				and 310 had loſt all their childr.		
1389				1389		

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* In order to account for the difference between the number of the women in theſe and the preceding tables, it is proper to mention, that this account was not begun until ſome months after the former one. In theſe alſo care has been taken that no woman is reckoned more than once, although many of them had been aſſiſted by the midwives to the Diſpenſary two, three, or four times. 370, as noted in the table, were in their firſt pregnancy.

† Of theſe 5419 children 2747 were boys, and 2672 girls, or nearly as 36 boys to 35 girls. This proportion of the boys to the girls will be found a little different from what is given in the table p. 362.

I have placed theſe two tables together, that we might have an opportunity of obſerving how exceedingly fertile the women of the poorer claſſes in this country are; and at the ſame time how unable to rear any conſiderable number of children; for, although 321 of the women had borne ſix children and upwards each, and were all again pregnant, 19 only of them had been able to rear ſix or more children; and, although 102 of the women had borne nine children and upwards each, only one of them had been able to preſerve that number living.

I am inclined to believe, that this great mortality amongſt the children does not ariſe from any natural imbecillity or a conſtitution vitiated from the birth, many of thoſe victims being born with all the appearances of health and vigour; but that we ought rather to ſearch for the cauſe of it in the poverty of the parents, which prevents their taking the neceſſary care of, or even affording ſufficient cloathing and nouriſhment to their offspring. Whether this great check to population is in its nature irremediable; and whether an abatement in the pariſh rates and taxes, but particularly the former, to perſons rearing more than a certain number of children, or any other mode of relief and encouragement, would contribute to reſtrain ſo melancholy an evil, are inquiries well deſerving the attention of government. In order to determine how well my conjectures on this ſubject are founded, it might be uſeful, perhaps, to learn what the proportion of deaths is in more opulent families, where the cauſe juſt now mentioned can have but little influence. But this muſt be the reſult of the united obſervations of different practitioners.

I ſhall now from theſe tables attempt to collect what the chance of life is at different periods, from infancy to twenty-ſix years of age; but, that I may be underſtood, it will be

neceſſary

neceſſary to premiſe ſome account of the method I have followed.

I have ſuppoſed each of the women to bear a child every two years; this, from the account of thoſe who returned to the Diſpenſary a ſecond, third, or fourth time, appearing to be the mean term. Upon this principle, when I find that a woman applied at the Diſpenſary who had had one child before, I conclude, that that child would be two years old, if living; but if the woman had borne two children, I ſuppoſe that the firſt would be four, the ſecond two years old, and ſo on. And finding, that of 299 children borne by as many women, who were now advanced in their ſecond pregnancy, 171, or ſeven-twelfths only were living, I conclude, that on an average 5 out of 12 die under two years of age: and obſerving that of 508 children borne by 254 women, who were now advanced in their third pregnancy, 259 only were living, I firſt deduct 210, which is five-twelfths of the whole number, who died under two years of age; and then find that 39, which is nearly one-twelfth of the whole number, or one-ſeventh of the ſurvivors, died between two and four years of age.

TABLE of the chance of life from infancy to 26 years of age.

Age.	Perſons living.	Decreaſe of life.	
0	5400	2250	5 in 12.
2	3150	450	6 in 12, or 1 in 7 of the ſurvivors.
4	2700	180	8 in 15, or 1 in 15 of the ſurvivors.
6	2520	204	4 in 7, or 1 in 12 and ½ of the ſurvivors.
8	2313	156	6 in 10, or 1 in 15 of the ſurvivors.
18	2160	540	7 in 10, or 1 in 4 of the ſurvivors.
26	1620		

3780 or ſeven-tenths would die.
1620 or three-tenths would be living at the end of 26 years.

5400

Whether this mode of calculating the chance of life will be admitted by gentlemen who have made ſpeculations of this kind their peculiar ſtudy, I know not. I confeſs, that when I firſt thought of it, I expected it would have proved more certain and accurate than upon examination I have found it to be: for, although in the firſt ſeries of years, where the deaths are numerous, the proportions agree tolerably well with the tables of M. **BUFFON** and others, yet as we advance we find ourſelves obliged to take longer periods than two years. Thus, for inſtance, we may obſerve, that although from 2 to 4, from 4 to 6, and from 6 to 8 years of age, the decreaſe continues to go on; yet ſo far is this from being the caſe between the ages of 8 and 10, or even 12, that there then appears to be ſome trifling increaſe. But as the proportion of deaths from 8 to 10 or 12 is probably inconſiderable, a very ſmall deduction from the deaths in the earlier years would rectify this difference. A deviation of this kind, I preſume, might be occaſioned by the ſmall-pox, or ſome other epidemic, prevailing amongſt children during two or three years of the time I was making this collection, which would occaſion the decreaſe in the firſt and ſecond ſeries to be greater than uſual. If this ſhould prove to be the caſe, it is probable, that in a courſe of years, by comparing this with a variety of ſimilar tables, the true medium may be found.

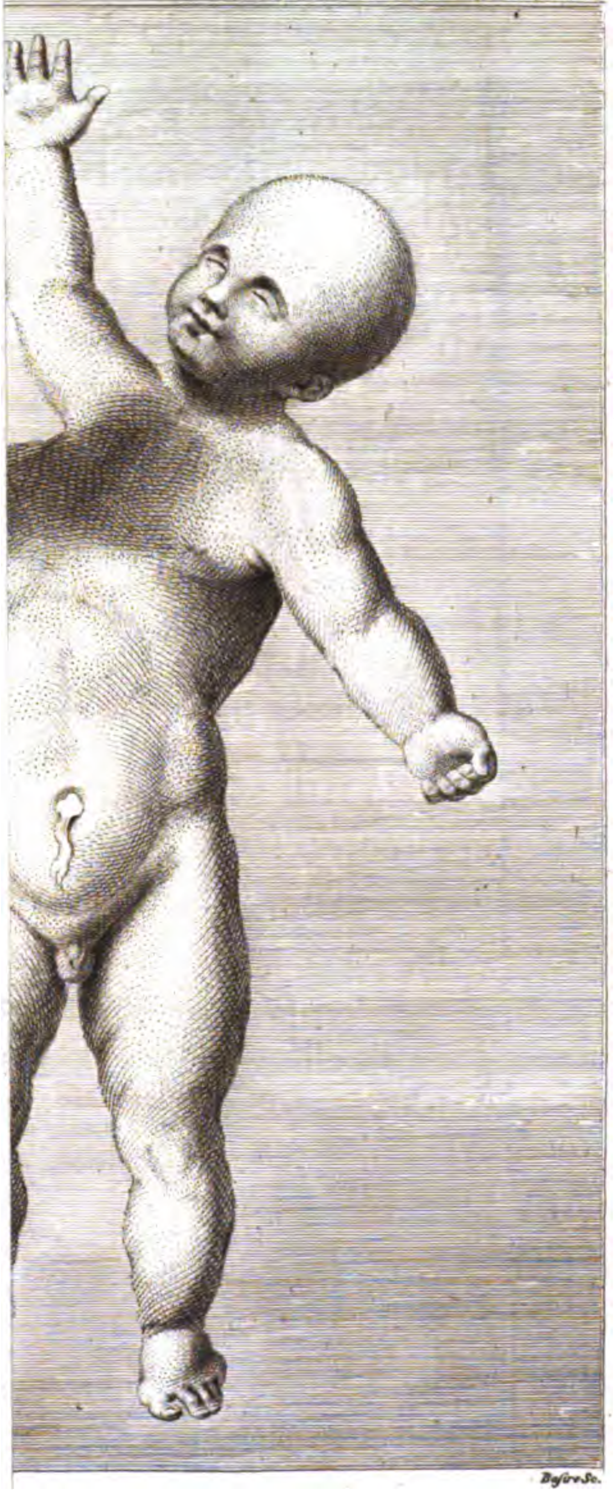
A COMPARATIVE TABLE of the population of London, with a view to ſhew the proportion of natives to perſons born in the different counties of England and Wales, in Scotland, Ireland, or foreign countries.

Of 3236 married perſons
824 or one-fourth were born in London.
1870 or four-ſevenths in the different counties of England and Wales.
209 or 1 in 15 in Scotland.
280 or 1 in 11 in Ireland.
53 or 1 in 60 were foreigners.
3236

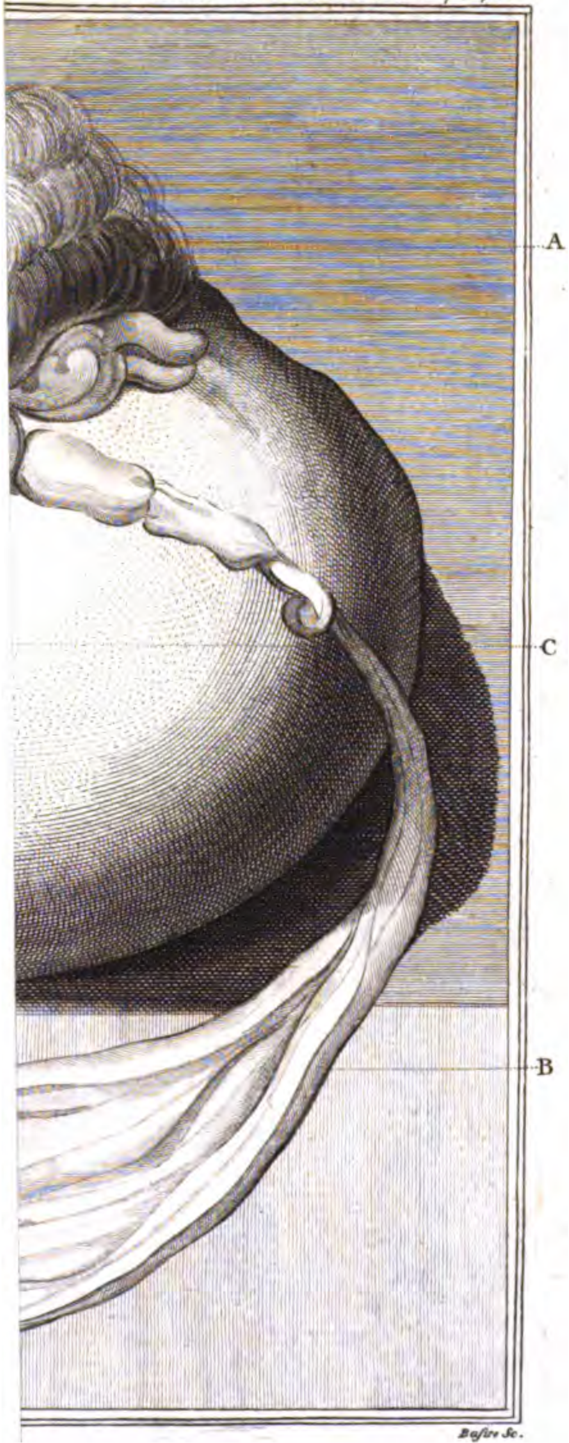
Of the above number the males and females were in the following proportions.

Men.		Women.	
329	were born in London, and	495	or 166 more than men.
952	—— in different counties	917	or 35 fewer than men.
135	—— in Scotland,	74	or 61 fewer than men.
162	—— in Ireland, —	119	or 43 fewer than men.
40	—— were foreigners,	13	or 27 fewer than men.
1618		1618	166

Philos. Trans. Vol. LXXI. Tab. XVII. p. 370.



Baſire Sc.


A
C
B
Basire Sc.

Thus, of 824 married perſons born in London, there were one-fifth more women than men. This may be accounted for either by ſuppoſing a greater number of males to die or to migrate before they attain a marriageable age than women. It is alſo to be obſerved, that of the Scotch and of the foreigners the women are in proportion to the men as about 1 to 3; but of the Iriſh they are as 3 to 7.

By this table we find at how great an expence to the country this city is maintained; and as we may ſuppoſe that the bulk of the Scotch, Iriſh, and foreigners, who come into the kingdom, reſide in the metropolis, we hence may alſo learn in what proportion they contribute to repair the waſte which is incurred by its exceſſive populouſneſs. A more compleat knowledge of theſe facts may give riſe to regulations which, if the calculations of Dr. PRICE ſhall be found to be juſt, are but too neceſſary; but I fear I have already intruded upon your patience, and extended this paper beyond its due bounds. I ſhall only add, that if theſe inquiries ſhould be favourably received by the illuſtrious body to whom you have ſo obligingly undertaken to preſent them, they will be continued, and their value of courſe increaſed by the additional number of objects which each year will ſupply.

I am, &c.

St. Alban's Street,
March 26, 1781.

* * *

XXIII. *Account of a Child who had the Small-pox in the Womb. In a Letter from* William Wright, *M. D. F. R. S. to* John Hunter, *Eſq. F. R. S.*

Read May 21, 1781.

Southampton-buildings, Holborn,
Feb. 27, 1781.

SIR,

I HAVE read with much pleaſure and information Mrs. FORD's caſe, which you publiſhed in Phil. Tranſ. vol. LXX. p. 128. From the facts you have adduced it amounts to a certainty, that her fœtus had received the variolous infection in the womb.

This induces me to lay before you a ſingular caſe, that fell under my care ſome years ago. I am ſorry I cannot be more particular, having unfortunately loſt all my books and my notes of practice of this caſe and ſeveral others, by the capture of the convoy on the 9th of laſt Auguſt.

In 1768 the ſmall-pox was ſo general in Jamaica that very few people eſcaped the contagion. About the middle of June Mr. PETERKIN, merchant at Martha-brae, in the pariſh of Trelawney, got about fifty new negroes out of a ſhip: ſoon after they landed, ſeveral were taken ill of a fever, and the ſmall-pox appeared; the others were immediately inoculated. Amongſt the number of thoſe who had the diſeaſe in the natural way, was a woman of about twenty-two years of age, and big with child. The eruptive fever was ſlight, and the ſmall-pox ſmall-pox had appeared before I ſaw her. They were few, diſtinct and large, and ſhe went through the diſeaſe with very little trouble, till on the fourteenth day from the eruption ſhe was attacked with the fever, which laſted only a few hours. She was, however, the ſame day taken in labour, and delivered of a female child with the ſmall-pox on her whole body, head, and extremities. They were diſtinct and very large, ſuch as they commonly appear on the eighth or ninth day in favourable caſes. The child was ſmall and weakly; ſhe could ſuck but little; a wet nurſe was procured, and every poſſible care taken of this infant, but ſhe died the third day after ſhe was born. The mother recovered, and is now the property of ALEXANDER PETERKIN, Eſq. in St. James's Pariſh.

In the courſe of many years practice in Jamaica, I have remarked, that where pregnant women had been ſeized with the natural ſmall-pox, or been by miſtake inoculated, that they generally miſcarried in the time of, or ſoon after, the eruptive fever; but I never ſaw any ſigns of ſmall-pox on any of their bodies, except on the child's above mentioned.

I am, &c.

XXIV. *Natural Hiſtory of the Inſect which produces the Gum Lacca. By Mr.* James Kerr, *of* Patna; *communicated by Sir* Joſeph Banks, *P. R. S.*

Read May 24, 1781.

COCCUS LACCA.

Head and trunk.

THE head and trunk form one uniform, oval, compreſſed, red body, of the ſhape and magnitude of a very ſmall louſe, conſiſting of twelve tranſverſe rings. The back is carinate; the belly flat; the antennæ half the length of the body, filiform, truncated, and diverging, ſending off two, often three, delicate, diverging hairs, longer than the antennæ. The mouth and eyes could not be ſeen with the naked eye.

Tail.

The tail is a little white point, ſending off two horizontal hairs as long as the body.

Feet.

It has three pair of limbs, half the length of the inſect.

I have often obſerved the birth of theſe inſects, but never could ſee any with wings; nor could I find any diſtinction of ſexes, nor obſerve their connubial rites: nature and analogy ſeem to point out a deficiency in my obſervations, poſſibly owing to the minuteneſs of the object, and want of proper glaſſes.

Change. This inſect is deſcribed in that ſtate in which it ſallies forth from the womb of the parent in the months of November and December. They traverſe the branches of the trees upon which they were produced for ſome time, and then fix themſelves upon the ſucculent extremities of the young branches. By the middle of January they are all fixed in their proper ſituations, they appear as plump as before, but ſhew no other marks of life. The limbs, antennæ, and ſetæ of the tail are no longer to be ſeen. Around their edges they are environed with a ſpiſſid ſubpellucid liquid, which ſeems to glue them to the branch: it is the gradual accumulation of this liquid, which forms a compleat cell for each inſect, and is what is called Gum Lacca. About the middle of March the cells are completely formed, and the inſect is in appearance an oval, ſmooth, red bag, without life, about the ſize of a ſmall cochanical inſect, emarginated at the obtuſe end, full of a beautiful red liquid. In October and November we find about twenty or thirty oval eggs, or rather young grubs, within the red fluid of the mother. When this fluid is all expended, the young inſects pierce a hole through the back of their mother, and walk off one by one, leaving their exuviæ behind, which is that white membraneous ſubſtance found in the empty cells of the Stick Lac.

Place.

Place.

The inſects are the inhabitants of four trees.

1. Ficus Religioſa, LINNÆI. In Hindoſtan, Pipul. Banyan Tree.

2. Ficus Indica, LINNÆI. In Hindoſtan, Bhur. Banyan Tree.

3. Plaſo Hortus Malabarici. By the natives, Praſo.

4. Rhamnus Jujuba, LINNÆI. In Hindoſtanick, Beyr.

The inſects generally fix themſelves ſo cloſe together, and in ſuch numbers, that I imagine only one in ſix can have room to compleat her cell: the others die, and are eat up by various inſects. The extreme branches appear as if they were covered with a red duſt, and their ſap is ſo much exhauſted, that they wither and produce no fruit, the leaves drop off, or turn to a dirty black colour. Theſe inſects are tranſplanted by birds: if they perch upon theſe branches, they muſt carry off a number of the inſects upon their feet to the next tree they reſt upon. It is worth obſerving, that theſe fig trees when wounded drop a milky juice, which inſtantly coagulates into a viſcid ropey ſubſtance, which, hardened in the open air, is ſimilar to the cell of the Coccus Lacca. The natives boil this milk with oils into a bird-lime, which will catch peacocks or the largeſt birds.

A red medicinal gum is procured by inciſion from the Plaſo Tree, ſo ſimilar to the Gum Lacca that it may readily be taken for the ſame ſubſtance. Hence it is probable, that thoſe inſects have little trouble in animalizing the ſap of theſe trees in the formation of their cells. The Gum Lacca is rarely ſeen upon the Rhamnus Jujuba; and it is inferior to what is found upon the other trees. The Gum Lacca of this country is principally found upon the uncultivated mountains on both ſides the Ganges, where bountiful nature has produced it in ſuch abundance, that was the conſumption ten times greater the markets might be ſupplied by this minute inſect. The only trouble in procuring the Lac is in breaking down the branches, and carrying them to market. The preſent price in Dacca is about twelve ſhillings the hundred pounds weight, although it is brought from the diſtant country of Aſſam. The beſt Lac is of a deep red colour. If it is pale, and pierced at top, the value diminiſhes, becauſe the inſects have left their cells, and conſequently they can be of no uſe as a dye or colour, but probably they are better for varniſhes.

This inſect and its cell has gone under the various names of Gum Lacca, Lack, Loc Tree. In Bengal, La; and by the Engliſh it is diſtinguiſhed into four kinds.

1ſt. Stick Lac, which is the natural ſtate from which all the others are formed.

2. Seed Lac is the cells ſeparated from the ſticks.

3d. Lump Lac is Seed Lac liquified by fire, and formed into cakes.

4th. Shell lac is the cells liquified, ſtrained, and formed into thin tranſparent laminæ in the following manner. Separate the cells from the branches, break them into ſmall pieces, throw them into a tub of water for one day, waſh off the red water and dry the cells, and with them fill a cylindrical tube of cotton cloth, two feet long, and one or two inches in diameter; tie both ends, turn the bag above a charcoal fire; as the Lac liquifies twiſt the bag, and when a ſufficient quantity has tranſuded the pores of the cloth, lay it upon a ſmooth junk of the Plantain tree (Muſa Paradiſiaca, LINNÆI), and with a ſtrip of the Plantain leaf draw it into a thin lamella; take it off while flexible, for in a minute it will be hard and brittle. The value of Shell Lac is according to its tranſparency.

Uſe to the natives.

This is one of the moſt uſeful inſects yet diſcovered.

Ornaments for the ladies.

The natives conſume a great quantity of Shell Lac in making ornamental rings, painted and gilded in various taſtes, to decorate the arms of the ladies; and it is formed into beads, ſpiral and linked chains for necklaces, and other female ornaments.

Sealing wax.

Take a ſtick, and heat one end of it upon a charcoal fire; put upon it a few leaves of the Shell Lac ſoftened above the fire; keep alternately heating and adding more Shell Lac, until you have got a maſs of three or four pounds of liquified Shell Lac upon the end of your ſtick [*]. Knead this upon a wetted board with three ounces of levigated cinnabar, form it into cylindrical pieces; and, to give them a poliſh, rub them while hot with a cotton cloth.

Japanning. Take a lump of Shell Lac, prepared in the manner of ſealing-wax, with whatever colour you pleaſe, fix it upon the end of a ſtick, heat the poliſhed wood over a charcoal fire, and rub it over with the half-melted Lac, and poliſh, by rubbing it even with a piece of folded Plantain leaf held in the hand; heating the lacquer, and adding more Lac as occaſion requires. Their figures are formed by Lac, charged with various colours in the ſame manner.

Varniſh. In ornamenting their images and religious houſes, &c. they make uſe of very thin beat lead, which they cover with various varniſhes, made of Lac charged with colours. The preparation of them is kept a ſecret. The leaf of lead is laid upon a ſmooth iron heated by fire below, while they ſpread the varniſh upon it.

Grindſtones. Take of river ſand three parts, of Seed Lac waſhed one part, mix them over the fire in a pot, and form the maſs into the ſhape of a grindſtone, having a ſquare hole in the center, fix it on an axis with liquified Lac, heat the ſtone moderately, and by turning the axis it may eaſily be formed into an exact orbicular ſhape. Poliſhing grindſtones are made only of ſuch ſand as will paſs eaſily through fine muſlin, in the proportion of two parts ſand to one of Lac. This ſand is found at Ragimaul. It is compoſed of ſmall angular cryſtalline particles, tinged red with iron, two parts to one of black magnetic ſand.

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[*] In this manner Lump Lac is formed from Seed Lac.

The ſtone-cutters, inſtead of ſand, uſe the powder of a very hard granite called Corune.

Theſe grindſtones cut very faſt. When they want to increaſe their power they throw ſand upon them, or let them occaſionally touch the edge of a vitrified brick. The ſame compoſition is formed upon ſticks, for cutting ſtones, ſhells, &c. by the hand.

Painting. Take one gallon of the red liquid from the firſt waſhing for Shell Lac, ſtrain it through a cloth, and let it boil for a ſhort time, then add half an ounce of ſoap earth (foſſil alkali); boil an hour more, and add three ounces of powdered load (bark of a tree); boil a ſhort time, let it ſtand all night, and ſtrain next day. Evaporate three quarts of milk, without cream, to two quarts, upon a ſlow fire, curdle it with ſour milk, and let it ſtand for a day or two; then mix it with the red liquid above mentioned; ſtrain them through a cloth, add to the mixture one ounce and an half of allum, and the juice of eight or ten lemons: mix the whole, and throw it into a cloth-bag ſtrainer. The blood of the inſect forms a coagulum with the caſeous part of the milk, and remains in the bag, while a limpid acid water drains from it. The coagulum is dried in the ſhade, and is uſed as a red colour in painting and colouring.

Dying. Take one gallon of the red liquid prepared as before without milk, to which add three ounces of allum. Boil three or four ounces of tamarinds in a gallon of water, and ſtrain the liquor. Mix equal parts of the red liquid and tamarind water over a briſk fire. In this mixture dip and wring the ſilk alternately until it has received a proper quantity of the dye. To increaſe the colour, increaſe the proportion of the red liquid, and let the ſilk boil a few minutes in the mixture. To make the ſilk hold the colour, they boil a handful of the bark called Lead in water, ſtrain the decoction, and add cold water to it; dip the dried ſilk into this liquor ſeveral times, and then dry it. Cotton cloths are dyed in this manner; but the dye is not ſo laſting as in ſilk.

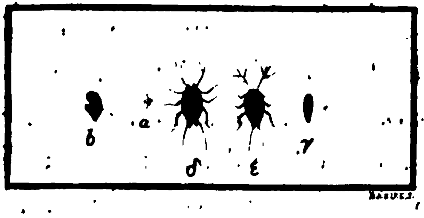
Spaniſh wool. The Lac colour is preſerved by the natives upon flakes of cotton dipped repeatedly into a ſtrong ſolution of the Lac Inſect in water, and then dried.

Uſe to the Europeans. See European authors.

Explanation of the figures.

a The Coccus Lacca at its birth,
b Ditto, big with young, } natural ſize.

γ The embrio before birth incloſed in its membrane,
δ The Coccus, with two hairs from each antenna,
ε Ditto, with three hairs from each antenna, } magnified.



XXV. *Account of a Phenomenon obſerved upon the Iſland of* Sumatra. *By* William Marſden, *Eſq.*; *communicated by Sir* Joſeph Banks, *P. R. S.*

Read May 24, 1781.

Caroline-ſtreet,
Feb. 24, 1781.

SIR,

DURING my reſidence on the iſland of Sumatra in the Eaſt Indies, I had occaſion to obſerve a phenomenon ſingular, I believe, in its kind, an account of which may not perhaps be unintereſting to the curious.

In the year 1775 the S.E. or dry monſoon, ſet in about the middle of June, and continued with very little intermiſſion till the month of March in the following year. So long and ſevere a drought had not been experienced then in the memory the oldeſt man. The verdure of the ground was burnt up, the trees were ſtripped of their leaves, the ſprings of water failed, and the earth every where gaped in fiſſures. For ſome time a copious dew falling in the night ſupplied the deficiency of rain; but this did not laſt long: yet a thick fog, which rendered the neighbouring hills inviſible for months together, and nearly obſcured the ſun, never ceaſed to hang over the land, and add a gloom to the proſpect already but too melancholy. The Europeans on the coaſt ſuffered extremely by ſickneſs; about a fourth part of the whole number being carried off by fevers and other bilious diſtempers, the depreſſion of ſpirits which they laboured under not a little contributing to haſten the fatal effects. The natives alſo died in great numbers.

In the month of November 1775, the dry ſeaſon having then exceeded its uſual period, and the S.E. winds continuing with unremitting violence, the ſea was obſerved to be covered, to the diſtance of a mile, and in ſome places a league from ſhore, with *fiſh* floating on the ſurface. Great quantities of them were at the ſame time driven on the beach or left there by the tide, ſome quite alive, others dying, but the greateſt part quite dead. The fiſh thus found were not of one but various ſpecies, both large and ſmall, flat and round, the Cat-fiſh and Mullet being generally the moſt prevalent. The numbers were prodigious, and overſpread the ſhore to the extent of ſome degrees; of this I had ocular proof or certain information, and probably they extended a conſiderable way farther than I had opportunity of making enquiry. Their firſt appearance was ſudden; but though the numbers diminiſhed, they continued to be thrown up, in ſome parts of the coaſt, for at leaſt a month, furniſhing the inhabitants with food, which, though attended with no immediate ill conſequence, probably contributed to the unhealthineſs ſo ſeverely felt. No alteration in the weather had been remarked for many days previous to their appearance. The thermometer ſtood as uſual at the time of year at about 85°.

Various were the conjectures formed as to the cauſe of this extraordinary phenomenon, and almoſt as various and contradictory were the conſequences deduced by the natives from an omen ſo portentous; ſome inferring the continuance, and others, with equal plauſibility, a relief from the drought. With reſpect to the cauſe, I muſt confeſs myſelf much at a loſs to account for it ſatisfactorily. If I might hazard a conjecture, and it is not offered as any thing more, I would ſuppoſe, that the ſea requires the mixture of a due proportion of freſh water to temper its ſaline quality, and enable certain ſpecies of fiſh to ſubſiſt in it. Of this ſalubrious correction it was deprived for an unuſual ſpace of time, not only by the want of rain, but by the ceaſing of many rivers to flow into it, whoſe ſources were dried up. I rode acroſs the mouths of ſeveral perfectly dry, which I had often before paſſed in boats. The fiſh no longer experiencing this refreſhment, neceſſary as it would ſeem to their exiſtence, ſickened and periſhed as in a corrupted element.

If any thing ſimilar to what I have above deſcribed has been noticed in other parts of the world, I ſhould be happy by a compariſon of the attendant circumſtances, to inveſtigate and aſcertain the true cauſes of ſo extraordinary an effect. In communicating to you the obſervations I have made, I purſue the moſt likely means of obtaining this ſatisfaction.

I have the honour to be, &c.

XXVI. *Farther Experiments on Cold, made at the* Macfarlane *Obſervatory belonging to* Glaſgow College. *In a Letter from* Patrick Wilſon, *M. A. to the Rev.* Nevil Maſkelyne, *D. D. F. R. S. and Aſtronomer Royal.*

Read May 24, 1781.

DEAR SIR, Glaſgow College, Feb. 9, 1781.

SOME days of very cold weather, which we had lately in this country, having afforded an opportunity of proſecuting a little farther the experiments and obſervations begun in the courſe of laſt year, I now do myſelf the pleaſure of communicating to you the following particulars, which perhaps may be conſidered as not unworthy of notice.

The froſt ſet in on Sunday the 21ſt of January, after a conſiderable fall of ſnow on the preceding evening, and about midnight the thermometers were expoſed near to the Obſervatory in the ſituations mentioned in my former letter. The following regiſter ſhews the difference of temperature between the ſnow and the air, till eight o'clock on Monday morning, to which are ſubjoined ſome facts which prove very conſonant to thoſe deſcribed in the former paper.

The ſign − prefixed denotes degrees below 0. The ſign + degrees above 0 of FAHRENHEIT's thermometer.

Monday

Monday morning, January 22, 1781.

h.	m.		Thermometer in air.		Thermometer on ſnow.
1		-	0	-	− 12
1	30	-	+ 2	-	− 12
1	45	-	0	-	− 8
2		-	− 2	-	− 7
3		-	0	-	− 4
3	45	-	0	-	− 8
4		-	− 1	-	− 12
4	30	-	− 3	-	− 8
5		-	− 2	-	− 12
6	15	-	− 3	-	− 13
6	45	-	− 2	-	− 10
7		-	− 3	-	− 13
7	30	-	− 2	-	− 10
8		-	− 4	-	− 11
8	30	-	− 2	-	− 10

From one o'clock till three in the morning the thermometer in air at the baluſtrade of the eaſt wing of the Obſervatory pointed from + 4 to + 6, and on the ſnow there from − 2 to 0. At half an hour after one the thermometer in air, twenty-four feet from the ground, and to the windward of the houſe, pointed to + 7, and at eight o'clock to + 1. At three o'clock the ſnow in the park, three inches below the ſurface, raiſed the thermometer to + 14, and at ſix inches below, near the ground, to + 24. The barometer ſtood at 29.8 inches, and there was a perceptible motion of the air from the eaſt and one point ſouth. This night was a very general and lively aurora borealis, moſt part of it of a bright red, which formed a crown near to the zenith; but it moſtly vaniſhed about three o'clock, after which time the air became more ſtill. During the whole of this night, as well as of the ſucceeding times of obſerving, the air was not nearly ſo much diſpoſed to give out hoar-froſt as it was laſt year.

On Monday evening the difference of temperature was found to be as in the following regiſter.

Monday evening,

h.	m.		Therm. in air.	Therm. in ſnow.
8		— — — —	+16	+7
8	30	— — — —	+14	+3
9		— — — —	+ 8	+1
9	30	— — — —	+ 7	+1
10		— — — —	+ 7	+3
10	30	— — — —	+ 6	+0
11		Ball of therm. ½ an inch above the ſurface of the ſnow,	+ 5	+3
12		Ditto, — — — —	+ 5	+3
Tueſday morning.				
1		Ball of therm. as formerly half immerſed in the ſnow,	+ 6	+3
2		— — — —	+ 8	+5
2	30	— — — —	+10	+6

No aurora this evening; the air very ſtill and ſerene till about two o'clock Tueſday morning, when the wind roſe remarkably, and clouds formed in the north-eaſt.

On Thurſday, January 25, the difference of temperature was found to be as here ſet down.

Thurſday morning.

h.	m.	Thermometer in air.	Thermometer in ſnow.
9	45	+ 10	+ 3
10		+ 10	+ 3
10	30	+ 14	+ 4
11		+ 14	+ 8
11	30	+ 17	+ 9
12		+ 20	+ 12
12	30	+ 22	+ 20
1	afternoon.	+ 25	+ 26
1	30	+ 27	+ 27

From ten till eleven o'clock this forenoon the thermometer on the baluſtrade in air, ſix inches above the ſnow, pointed to + 14, and when tried upon the ſnow to + 10. About noon this day ſome clouds were formed, which became quite general by one o'clock.

During the two laſt times of obſerving, three experiments were made with a view of diſcovering whether the ſnow without doors was gaining any thing from the air; or if any of it was carried off in the way of evaporation? For this purpoſe, a ſhallow diſh, made of ſheet braſs, four inches in diameter, was exactly filled with ſnow, and carefully weighed. In order to defend the outſide of the diſh from the air, that no hoarfroſt might attach itſelf to the metal, a circular hole was cut in the lid of a paſte-board box, ſo wide as juſt to let in the diſh to the very brim, ſo that nothing communicated with the external air but the ſnow itſelf. The apparatus, in this ſtate, was ſet without doors for three hours each time, and then brought in to the lobby of the Obſervatory, where the diſh was again weighed: but in none of theſe trials did it ever appear, that any weight was loſt. On the contrary, at the firſt weighing, which was on Monday night, twelve o'clock, it had gained five grains. In the other two trials the increaſe of weight was ſcarce perceivable.

The temperature of the air in the weſt room of the Obſervatory remaining very conſtantly for near two days at +27°, a diſh of ſnow, ſimilar to the other expoſed there, was found to loſe weight very ſenſibly, and for the moſt part at the rate of two grains in an hour. Notwithſtanding this, the ſnow thus waſting or evaporating had no power of ſinking the thermometer below +27, the temperature of the ſurrounding air; though at one time it was fanned for four minutes by a piece of paper faſtened to the end of a long ſtick. Not to diſturb the uniform temperature of this room during theſe experiments, care was taken to ſtay in it a very ſhort time at every viſit, and to keep the door and the window-ſhutters cloſe.

On Chriſtmas-day we had a froſt, which in the morning made the thermometer in air point to +21; and during the preceding night there had been a profuſe depoſition of hoar-froſt. A pound of this was collected, and its capacity for heat compared to that of ice, and found equal as nearly as could be judged. Before making the two mixtures neceſſary for this experiment, the ice was reduced to a powder, and ſpread out on a paper beſide the hoar-froſt till both had acquired the ſame temperature.

On Monday night, January 22, about twelve o'clock, having occaſion to take up a little ſnow, there was obſerved a coheſion among its parts rather greater than what might have been expected in a ſubſtance, at that time, ſo much frozen. This circumſtance was farther examined by the following experiment. A pane of glaſs was laid on the ſurface of the ſnow till it had acquired the temperature of +3, after which, with a bit of parchment equally cold, ſome ſnow was ſcraped from the very ſurface, and ſhaken all over the pane, ſo as to cover it in moſt parts lightly. Upon now lifting the pane, and holding it with the ſnow undermoſt, the whole of it adhered, and it required ſome ſmart raps before the greater part fell away. What remained cleaved to the glaſs with ſtill a greater adheſion.

The experiments related above afford further reaſons againſt the opinion of the difference of temperature betwixt the ſnow or hoar-froſt and the air depending upon evaporation. It would moreover appear, that this phenomenon depends not either upon the depoſition of hoar-froſt. What renders this the more probable is, that on laſt year there was a much more copious depoſition at times when the difference of temperature was not more remarkable. But allowing that a depoſition had been found a neceſſary circumſtance, and always in proportion to that difference, the experiments on the capacities of hoar-froſt and ice ſeem to ſhew, that the ſenſible heat which diſappears enters not into the compoſition of the hoar-froſt; otherwiſe the capacity of this ſubſtance for heat, compared to that of ice or common ſnow, ſhould be very different. It muſt be confeſſed, however, that the above mentioned experiment would have been more applicable to this reaſoning, had it been made with hoar-froſt given out in colder ſtates of the air.

If the air, at low temperatures, had any power of acting upon the ſnow or hoar-froſt, ſo as to produce a gradual melting, this circumſtance, according to the known laws of heat, might occaſion the difference of temperature under conſideration. And what renders this idea not altogether improbable, is the peculiar coheſion among the parts of the ſnow above deſcribed. Perhaps a gentle melting might take place without much altering the appearance of the ſnow or hoar-froſt at the ſurface, as the parts, when diſſolved, might be gradually ſucked downwards, and be afterwards diſtributed through the whole drier maſs. It may alſo be worthy of an experimental inquiry to determine, how far that ſort of concretion, obſervable all over the ſurface of ſnow which has been long frozen, bears any marks of a ſlow proceſs of this kind. From a hill, a little way to the N.E. of the town, and which was to windward during the froſt, there were gathered two portions of ſnow, the one from the ſurface, and the other ſeven inches below it. The water produced from the two kinds is preſerved in very clean phials, in order to be compared together by ſome chemical trials, which, perhaps, may throw ſome light upon the whole of this matter.

At preſent I ſhall conclude this letter, perhaps already much too long, with juſt mentioning one other fact which was new to me; namely, the power of ardent ſpirits of diſſolving ſnow, and conſequently of producing with it a freezing mixture. The alcohol and ſnow ſeparately were at eight degrees below the freezing point, and when mixed ſuddenly and intimately, the temperature became in the ſpace of twenty ſeconds 28° below 0. This is a cold only 12° ſhort of that which FAHRENHEIT firſt produced by uſing ſpirit of nitre for the experiment; and it is not improbable, had the preſent experiment been tried with more precaution and addreſs, that the reſult would have been ſtill more remarkable. There was employed only about a pint of alcohol, but the proportion of ſnow was not then attended to, and the thaw coming ſoon afterwards prevented a repetition of the experiment.

I am, &c.

POSTSCRIPT.

I beg leave to add, that the water mentioned as produced from the ſuperficial ſnow has been examined by ſeveral chymical trials, with a view of diſcovering if it differed in any reſpect from the water obtained from ſnow gathered at conſiderable depths, and near the ground. Had the atmoſphere, when the thermometers pointed ſo low, been diſpoſed to furniſh any ſaline principle, the union of ſuch an ingredient with the ſnow would have tended to produce an exceſs of cold at the ſurface, ſimilar to what was then obſerved. Or if the ſnow at theſe low temperatures had acquired any remarkable power of dephlogiſticating the air in contact with it, a cooling proceſs at and near the confines of the ſnow and air might thereby have been maintained. In either of theſe caſes, ſome very ſenſible indications of a ſaline or of a phlogiſtic principle might be expected on the water given by the ſnow collected from the ſurface. But in oppoſition to both theſe views it remains now to be mentioned, that nothing of this kind did appear in the courſe of the experiments, which indeed were contrived chiefly to detect ſuch circumſtances.

If therefore the arguments produced in both papers upon this ſubject will not allow us to account for ſo remarkable a cooling proceſs by an evaporation at the ſurface of the ſnow, it would appear, that there remains ſtill ſomething unknown with reſpect to the cauſe. A proper inveſtigation of this matter, in climates favourable to ſuch experiments, may poſſibly unfold ſome farther properties of heat with which at preſent we may be wholly unacquainted.

XXVII. *A general Theory for the Menſuration of the Angle ſubtended by Two Objects, of which One is obſerved by Rays after Two Reflections from plane Surfaces, and the other by Rays coming directly to the Spectator's Eye.* By George Atwood, *M. A. F. R. S.*

Read June 21, 1781.

THE actual determination of an angle implies two obſervations, one taken at each extremity of the arc by which that angle is meaſured. When fixed aſtronomical quadrants or other ſectors are uſed for the practical eſtimation of angles, one of theſe obſervations is previouſly made by directing the axis of the teleſcope or line of collimation to ſome fixed point in the heavens, the index being then coincident with the initial point on the arc of the ſector: after this adjuſtment one obſervation only is neceſſary to aſcertain the angular diſtance between that point and any other celeſtial object in the plane of the ſector. This method, however, is evidently impracticable, unleſs the inſtrument can be ſteadily fixed; for which reaſon aſtronomical quadrants become uſeleſs at ſea; and from the difficulties which attend placing them in their due poſition and adjuſtment on firm ground, they are almoſt wholly confined to regular obſervatories.

Mr. HADLEY *, by an ingenious application of optical principles, contrived to bring both extremities of the arc meaſured into the field of the ſpectators's view at the ſame time; by which improvement, angles are taken at ſea, as well as on land with an unfixed inſtrument, to a degree of accuracy ſufficient for nautical and other purpoſes, when the utmoſt exactneſs is not required.

Mr. HADLEY's invention is a particular caſe of a very extenſive theory, as yet but little attended to. According to his method, which is well known, the two reflecting ſurfaces uſed in the obſervation are perpendicular to the plane of motion; the direction of the teleſcope, and of the rays paſſing between the reflectors being parallel to that plane; whereas the inclination of the teleſcope, and of the intermediate rays, as well as of the reflectors themſelves to the plane of motion, admits of unlimited variety. A general theory to determine the angle obſerved by two reflections from the data on which its magnitude depends, without limitation or reſtriction, ſeems applicable to ſeveral uſeful purpoſes in practical aſtronomy. Having never ſeen any geometrical conſtruction or analyſis of this curious problem, I was induced to beſtow ſome conſideration

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* Phil. Tranſ. N° 420. See alſo a tract, intituled, The Theory of HADLEY's quadrant, by the rev. W. LUDLAM.

on the ſubject, and ſhall be happy if the reſult of my inquiries appears to merit the attention of the Royal Society.

Art. 1. The manner of taking an obſervation by two reflections unconfined to any particular caſe may be deſcribed thus. Let C, B (fig. 1.) repreſent two plane reflecting ſurfaces, inclined to a plane OPA at any given angle. Through any point of the reflecting ſurface C draw a line perpendicular to the plane OPA, and with the point where the line meets the plane as a centre (which muſt here be repreſented by C) and any diſtance CP, deſcribe a circle OPA. The reflecting plane B always continuing fixed, let the reflector C be moveable along with the radius CP as it revolves in the plane OPA round the centre C: the angular motion of the ſpeculum C, referred to the circumference OPA, will be meaſured by the arc which the radius CP deſcribes, the inclination of the plane C to the plane of motion OPA being always the ſame, and equal to that of the fixed ſpeculum B.

2. The two plane reflectors, B and C, being equally inclined to the plane OPA, it follows, that during the motion of C there muſt be ſome point O in the circumference OAP, at which when CP arrives, the reflector C will be parallel to the fixed reflector B.

3. When the moveable radius which carries round the plane C is at any other poſition CP, let a ray flowing from a diſtant object T impinge on the ſpeculum C; let it be reflected from thence in the direction CB, and being again reflected at B in the direction BG, let it be obſerved by a ſpectator's eye at G; the image of T will appear ſomewhere in the line GBS; ſuppoſe that a ray flows from a diſtant object S ſituated in the line GB produced, and that this ray SG comes directly to the ſpectator's

eye at G: the object S ſeen by direct rays, and the image of the point T ſeen by rays after two reflections, will appear to coincide in the line GBS. This is an obſervation by two reflections, from which, together with ſuch data as limit the problem, the true angle ſubtended by the objects T and S is to be inferred.

4. The data which limit this problem, being neceſſary for the determination of the angle ſubtended by T and S are in number four, which are next to be conſidered. 1ſt. One of theſe data is the arc PO, being the angular diſtance of the moveable radius CP, meaſured on the circumference of the circle OPA, from that poſition CO, at which the two reflectors are parallel; the ſituation of this arc OP in reſpect of the point O being ſuppoſed known, that is, it being known on which ſide of that point, OP is ſituated in reſpect of the ray BG. 2dly, The common inclination of the reflecting planes B and C to the plane of motion is another of theſe data. The third and fourth of the conditions muſt be mentioned rather more particularly. The ray BG is always underſtood to be given in poſition in reſpect of the plane of motion OPA (conſidered as immoveable) being either coincident with the line of collimation of a teleſcope, or directed by ſights ſo as to be invariably fixed: the ſpeculum B alſo being unmoved, the line or ray BC will never change its poſition, from the known principles of reflection. The angle CBG, therefore, and the half of that angle being the angle of incidence at which CB impinges on B, will be always of the ſame magnitude; whereas the half of the angle BCT, or the angle of incidence on the moveable ſpeculum C, is continually changing, while C is carried round in the plane of motion: this conſtant angle of incidence or reflection at the fixed ſpeculum B will be another of the data neceſſary to determine the problem.

The rays GB, BC, and the ſpeculum B, being fixed in reſpect of each other, and of the plane OAP, the plane CBG will alſo be given in poſition; that is, its inclination to the plane of motion, or to any other fixed plane, will conſtantly be the ſame: whereas the inclination of the plane BCT to the plane of motion, or other fixed plane, will be continually changing while the reflector C revolves with the radius CP. The poſition of the plane GBC conſtitutes the fourth and laſt of the data, and it will be immaterial to what fixed plane it is referred. In the enſuing ſolution the ſituation of this plane will be defined by its inclination to the fixed ſecondary of the plane of motion which paſſes through the point O.

5. The enumeration of theſe data leads to the conſtruction of the problem, a few obſervations being previouſly inſerted to prevent repetitions and unneceſſary references. 1ſt, The objects obſerved are underſtood to be lucid or illumined points, and ſo diſtant, that the rays which flow from either of them may be eſteemed parallel without error as far regards theſe obſervations: ſuch objects are the fixed ſtars, any given points in the diſks of the ſun or planets, &c. 2dly, As in meaſuring the angular poſitions of objects which lie in the ſame plane, theſe objects are referred to the circumference of a circle, the centre of which is coincident with the ſpectator's eye; ſo in eſtimating the poſitions of objects which lie in different planes, and of the inclinations of theſe planes to each other, the objects, &c. are referred to the circumference of a ſphere, of which the centre coincides with the centre of the ſpectator's view: applying this to the preſent caſe, ſince the lines CT, CB, SG (fig. 1.) are ſituated in different planes; in order to eſtimate their poſitions, any point may be aſſumed as the centre of a ſphere, and through that point lines are to be drawn parallel to the given lines CT, CB, SG, the points in

which the lines interſect the ſpheres ſurface will give their relative ſituations by the rules of trigonomety. 3dly, There will be no neceſſity to repreſent the reflecting planes in the general conſtruction, ſince the poſitions of the perpendiculars to the planes will give the ſituations of the planes themſelves.

6. To determine by conſtruction the angle ſubtended by the objects T, S, from the data which have been deſcribed, let APOCQ (fig. 2.) repreſent a great circle of the ſphere to the ſurface of which the objects obſerved, and the poſitions of the incident and reflected rays, &c. are referred; C being the center, CK the axis, and K the pole of this great circle; through K draw any ſecondary KO, and from the pole K, at the diſtance of the arc KF, = the meaſure of the given inclination of the reflecting planes to the plane of motion, deſcribe a parallel or leſſer circle FIM: with the pole F, and at a diſtance equal to a quadrant, deſcribe an arc of a great circle interſecting the ſecondary KO produced in the point X, and in this arc from X take XY = the meaſure of the given inclination of the fixed plane of reflection at the ſpeculum B to the ſecondary which paſſes through the point O; and draw the quadrant YF, which produce in the direction YF: from F on either ſide of F ſet off FD equal to the meaſure of the given conſtant angle of incidence at the ſpeculum B, and make FB (taken on that ſide of F which is oppoſite to D) equal to FD. Draw the radius CO: from O ſet off an arc OP in the circumference OPA equal to the meaſure of the angular diſtance deſcribed by the moveable radius CP from that poſition at which the reflectors are parallel; obſerving that the arc OP be on that ſide of the point O which* correſponds with the conditions of the problem (art. 4.): through P deſcribe the ſecondary KP interſecting the parallel FIM in the point I: through B and I deſcribe

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* It is ſuppoſed to be known, whether CP beginning its motion from the poſition CO approaches towards the viſual ray BG or recedes from it.

the arc of a great circle BIE, and in it take EI equal to IB: through D and E draw the arc of a great circle DE: the arc DE will be the meaſure of the true angle ſubtended by the objects obſerved, according to the data of the problem.

Previous to the demonſtration of this conſtruction, the application of it to the method of obſervation by two reflections ſhould be deſcribed. Join CP, CI, and CF. To the extremity C of the radius CP let a plane ſpeculum be affixed, CI being always perpendicular to this plane: as PC revolves in the plane of motion, the perpendicular CI will deſcribe the parallel or leſſer circle FIM, and when CP coincides with CO, CI will coincide with CF. Through B draw BR parallel to CF, and let a plane ſpeculum be fixed at B perpendicular to BR; CF and BR being parallel when the perpendicular CI coincides with CF, the reflectors at C and B will then be parallel.

Join CD, and produce it to a very diſtant point S, and through B draw GBS parallel to CDS; the reflectors C and B being parallel, and their perpendiculars coinciding with CF and BR, let a ray SC impinge on the reflector C: becauſe FC is the perpendicular to the ſpeculum C and the arc DF = FB by conſtruction, theſe arcs being in the plane of the ſame great circle DBQ, it follows, that the ray SC will be reflected from C in the direction CB, impinging on the ſpeculum B at the angle of incidence CBR; and ſince DC and BG are parallel by conſtruction, and the parallel lines FC BR fall on them, the angles RBG, FCD, will be equal, and FCB or CBR = RBG. CB therefore being the ray incident on the ſpeculum B will be reflected in the direction BG parallel to SC; and a ray SG coming directly from S will be ſeen coincident with the reflected ray BG. Here we obſerve, that the planes of reflection at C and B, that is, the planes DCB and CBG coincide; the reflectors being parallel.

Let the radius CP move from the poſition CO, carrying with it the ſpeculum C and its perpendicular CI: then, EI being equal to IB by conſtruction, a ray impinging on C in the direction TEC will be reflected in the plane ECB, and becauſe ECI = ICB, the reflected ray will coincide with the line CB, and after reflection at B will * proceed in the direction BG, being coincident with the ray SG which comes directly from S. When the perpendicular CI leaves CF, the plane of reflection ICB becomes inclined to the plane of reflection DCBG with which it before coincided; but the poſition of the rays CB, BG, and of the perpendicular BR, remains unaltered; for which reaſon the plane GBCFD correſponds to the fixed plane of reflection deſcribed among the conditions (art. 4.). When CI was coincident with CF, the radius CP was coincident with CO, O being the initial point of the arc OP, deſcribed by the radius CP, denoting that when CP coincides with O, the reflectors being then parallel, the inclination of the ray SC obſerved after two reflections, and SG obſerved by direct rays parallel to SC, is nothing: the great circle KO, therefore, which paſſes through O and F, will be the fixed or primitive ſecondary to which the inclination of the fixed plane of reflection at the ſpeculum B is referred.

The demonſtration of the conſtruction will conſiſt of two parts. It muſt be firſt ſhewn, that the conditions or data of the problem are obſerved in the conſtruction. 2dly, That the magnitude of the arc ED, which meaſures the angle ſubtended by the obſerved objects is limited or determined by them.

Suppoſing the angle TCS to be of any unknown quantity, it has appeared, that according to the conſtruction, the rays which come from T, and are ſeen after two reflections at C and B, will be obſerved to coincide with the rays which come

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* Supra.

directly from S. That the conditions of the problem are fulfilled in the conſtruction is demonſtrated thus:

1ſt, The inclination of the reflectors B and C to the plane of motion was conſtructed of the magnitude which is meaſured by the arc KF. KC is perpendicular to the plane of motion. CF is perpendicular to the reflector C, and the inclination of theſe two lines CK, CF, is meaſured by the arc KF; but the inclination of any two planes is the ſame as the inclination of two lines which are perpendicular to them; the inclination therefore of the reflector C to the plane of motion is meaſured by the arc KF, and the ſpeculum B is equally inclined to the plane of motion with C by the conſtruction, the perpendiculars CF and CI being parallel when both are ſituated in the plane of the ſame great circle DBQ.

2dly, KO being the ſecondary to which the poſition of the fixed plane of reflection DFB at the ſpeculum B was referred, that given inclination will be equal to the angle OFB, which is meaſured by the arc XY according to the conſtruction, FY being a quadrant.

3dly, Moreover, FD = FB, was conſtructed equal to the conſtant angle of incidence at the fixed ſpeculum; CBR is the angle of incidence at the fixed ſpeculum B, and it is equal to the angle BCF, becauſe CF and BR are parallel by conſtruction, and CB falls on them; FB, or its equal FD therefore is truly conſtructed the meaſure of the given conſtant angle of incidence at the fixed ſpeculum B.

4thly, Becauſe it has been ſhewn that CO is the poſition of the radius CP, when the reflectors are parallel, the arc OP is rightly conſtructed the meaſure of the angular diſtance of the radius CP from that poſition.

It remains only to demonſtrate that theſe four given quantities, KF, OP, XY, and DF, limit the magnitude of the arc

ED: through I and F draw the arc IF: then the given arc KF or KI, and the angle IKF, meaſured by the given arc PO, define the triangle IKF, and in it, therefore, the ſide IF and and the angle IFK are determined. If from IFK, the given angle DFK, meaſured by the arc XY, be ſubtracted, the remainder IFD, and IFB its ſupplement to 180° will be defined: the given arc FB, with the angle IFB and the arc IF, determine the angle IBF, and the arc IB, or its double BE: and the given arc BD, the arc BE, with the contained angle DBE before determined, define the arc ED, which is therefore the true meaſure of the angle ſubtended by the objects obſerved under the conditions fulfilled in the conſtruction.

7. The computation of the obſerved angle DCE being for the preſent omitted, ſome conſequences which follow from the conſtruction may be inſerted in this place, being either corollaries, or ſuch truths as admit of eaſy geometrical deduction from the general propoſition. The line DC will always be the poſition of the viſual ray or line of collimation of the teleſcope uſed in the obſervation, and the inclination of it to the plane of motion will be meaſured by the complement of the arc DK to a quadrant. The line BC will be the poſition of the ray which paſſes between the reflectors B and C, and the inclination of it to the plane of motion will be meaſured by the complement of the arc BK to a quadrant. Theſe arcs are left out of the figure, that the more material parts of the conſtruction might not be confuſed by them.

8. Every thing elſe remaining, let the parallel FIM (fig. 3.) be projected on the plane of motion QOP. Through the points F and I draw the arc of a great circle NIFR. The obſerved * objects T and S, or, which is the ſame thing, the points of interſection at the ſphere's ſurface E and D will be at equal perpen-

* Compare fig. 2.

perpendicular diſtances from this arc, which may be demonſtrated thus. Through the points E, D, and B, draw the arcs EN, DL, and BR, perpendicular to NIFR: then the triangles DFL, FBR, being equal, DL will be equal to BR; moreover, the triangles ENI, IRB, being equal, the arcs EN, RB, will be equal: from whence it follows, that EN = DL, or the perpendicular diſtances of the points E and D from the arc of a great circle which paſſes through the points I and F, are equal. It appears alſo, from the ſame conſtruction, that the arc NL, intercepted between the two perpendiculars EN, DL, is equal to twice IF: for becauſe the triangles EIN, RIB, are equal, as are the triangles DLF, RFB, it follows, that NI is equal to IR, and LF to FR, wherefore 2IR = NR, and 2RF = LR: whence, by ſubtracting equals from equals, 2RI − 2RF = NR − LR, or 2IF = NL, which was the equality to be demonſtrated.

9. From this laſt conſtruction and demonſtration the following proportion is inferred. As radius : coſine of DL or EN, ſo is the ſine of IF to the ſine of half the arc ED, or of half the obſerved angle: for if the arcs NE, LD (fig. 3.), be continued until they meet in the pole H, the arcs NH, LH, will be quadrants, and the triangle EHD iſoſceles, which, from a property of ſpherics too obvious to need demonſtrating, gives this proportion: as the chord of NL to the chord of ED, ſo is radius to the ſine of DH, or coſine of DL; but the chord of NL is equal to the chord of 2IF from art. 8. We have, therefore, as radius : coſine DL, ſo is the chord of 2FI to the chord of ED, or, which is the ſame proportion, as radius : coſine DL, ſo is the ſine of IF to the ſine of half ED.

10. From the laſt article it appears, that the ſine of half the angle between the obſerved objects, or the ſine of half ED,

is proportional to the ſine of FI and the coſine of DL jointly; conſequently the ſine of FI being the ſame, ſin. ½ ED is proportional to the coſine of DL; this will lead to the reaſon why in enumerating (art. 4.) the* conditions which limit the magnitude of the obſerved arc ED, the poſition of the ſecondary KP, in reſpect of the point of interſection Q and of the fixed ſecondary KO, was annexed: for it will appear, that every thing elſe being the ſame, the magnitude of the arc ED will depend on the poſition of the ſecondary KP, whether it be on one ſide of the fixed ſecondary KO, or on the other, the angles PKO, *p*KO, being equal. Having ſet off O*p* = OP draw the ſecondary K*p* interſecting the parallel FIMU in the point U; and through B and U draw the arc of a great circle BUW; take UW = BU; and through D and W draw the arc of a great circle DW: then by the conſtruction and demonſtration in art. 6. the angle ſubtended by the obſerved objects will be meaſured by the arc DW, and it will be eaſy to ſhew, that DW is not equal to DE, except in two extreme caſes; that is, when the fixed plane of reflection DFB is either coincident with the primitive ſecondary KO or perpendicular to it. Through the points F and U draw the arc of a great circle VFU, and from D draw the arc DV perpendicular to VFU: ſince † the ſines of half the arcs DE, DW, are in a proportion compounded of the proportions of the ſine of IF to the ſine of FU, and of the coſine of DL to the coſine of DV, the ſines of IF, FU, being equal by the conſtruction, the ſines of half the arcs ED, DW, will be in the ſame proportion with the coſines of DL and DV, which are evidently unequal; conſequently, the ſines of half the arcs DE, DW, and therefore the arcs themſelves, muſt be unequal.

11. The angles PKO, OK*p*, remaining equal, when the fixed plane of reflection BFD (fig. 4.) is coincident with the

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* Compare fig. 2.

† Supra.

ſecondary KO, or at right angles to it, the perpendiculars DL, DV, become equal in both caſes, which is obvious from the equality of the triangles DVF, DLF; it follows, therefore, (art. 10.) that the ſines of FI and FU, and the coſines of DL, DV, being equal, the arcs DE, DW, will be equal in theſe two extreme caſes, but in no other.

12. Since the angle ſubtended by the obſerved objects (art. 10.) depends only on the ſine of IF and the coſine of DL, it is plain, that if the points D and B be interchanged, (fig. 2, 3, 4.) the angle obſerved will not be altered, every thing elſe remaining the ſame; becauſe neither the ſine of IF, nor the coſine of DL, is affected by this change. For this reaſon in any conſtruction for meaſuring angles by two reflections, the poſition of the * viſual ray may be altered into that of the ray BC paſſing between the reflectors, which will become in that caſe the ſituation of the viſual ray, this alteration noways affecting the obſerved angles.

13. While the perpendicular CI (fig. 2. and 5.) deſcribes the parallel FIM, the angle of incidence on the moveable ſpeculum C, that is, the angle ECI or ICB, meaſured by the arc BI, continually increaſes until it arrives at a certain limit. This limit is determined by drawing through the points B and K the arc of a great circle BKM. When the perpendicular CI arrives at M, the arc BM is the greateſt poſſible, which will therefore be the meaſure of the greateſt angle of incidence on the moveable ſpeculum, according to this conſtruction, the radius CP having then deſcribed from O an arc which is the meaſure of the angle FKM. Now it is plain, that if the arc MB ſhould be greater than a quadrant, there can be no viſion by two reflections, when the perpendicular CI coincides with M (ſuppoſing the moveable ſpeculum to reflect on one ſide only) becauſe the angles of incidence and reflection on any ſpeculum

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* The poſition of the ray DC is the ſame with that of the ray BG parallel to it, when referred to diſtant objects.

muſt be leſs than 90°. If BM be leſs than a quadrant, an obſervation by two reflections may be taken when the radius CP is directed to any point in the circumference of the plane of motion. When the arc BM is greater than a quadrant, two other limits will be produced in the circumference of the plane OCP; while the radius CP is between theſe limits, no obſervation by two reflections can be taken: theſe limits are conſtructed thus (fig. 6.). BM being greater than a quadrant, with the pole B and diſtance BI equal to a quadrant, deſcribe the arc of a great circle I*i* interſecting the parallel FIM in the points I and *i*: through I and *i* draw the ſecondaries KY, KZ: while the radius CP is between Z, and Y no obſervation can be taken by two reflections. If BIE, B*i*E, be drawn equal to a ſemi-circle, and DE joined, then DE will be the meaſure of the limiting angle which can be obſerved by this conſtruction, either on one ſide of KO or on the other; and becauſe, by the principles of trigonometry, the arcs BD and DE are in the ſame great circle, BDE being a ſemi-circle, we ſhall derive from the conſtruction this concluſion: the difference between 180° and double the angle of incidence on the fixed ſpeculum, will be a limit which terminates the angle obſerved by two reflections in every caſe, when the arc BM is greater than a quadrant *.

14. In any given example formed on the principles which have been demonſtrated (fig. 2.) for the eſtimation of angles by two reflections, three of the four quantities neceſſary to determine the reſult muſt conſtantly be the ſame, while the fourth, that is, the arc OP, varies with the magnitude of the angle ſubtended by the objects obſerved: the different magnitudes of theſe three given quantities will cauſe a great variety of properties in conſtructions which depend on the general

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* This termination of the angle which can be obſerved by two reflections may happen while the obſerved angle is increaſing or decreaſing during the revolution of the index in the plane of motion.

theory. If the angle DFK (fig. 2.), being the inclination of the fixed plane of reflection to the primitive ſecondary be = 90°, and the arc KF, or the inclination of the reflectors to the plane of motion, be = 90° alſo, the conſtruction will become that of HADLEY's inſtrument (fig. 7.), whatever be the magnitude of the arc DF, that is, of the angle of incidence on the fixed ſpeculum B: in this caſe the points F and O, and the points I and P, coincide. Here IF or PO meaſures the inclination of the reflectors to each other; and becauſe BF = FD, and BI = IE, by conſtruction, it follows, that DE = 2PO, that is, the angle ſubtended by the obſerved objects is double to the angle at which the reflectors are inclined to each other. This is a known property of HADLEY's inſtrument, in which the viſual ray, and the ray intermediate between the reflectors, are in the plane of motion, which is alſo expreſſed in the conſtruction, DC and BC coinciding with the plane POC.

15. Biſect KO in F; then will KF = 45° (fig. 8.). The viſual ray CD being coincident with the plane of motion, let the inclination of the reflectors to that plane be equal to 45°: moreover, let the angle * DFK = 180°; ſo ſhall D coincide with O, and B with K: this will afford a good example to the general theory. Let the radius CP move into any given poſition, carrying with it the ſpeculum C and its perpendicular CI: here the obſerved object E and the point B are always equi-diſtant from I; and becauſe BI is half a quadrant, by conſtruction, it follows, that IE will be of the ſame magnitude, BE therefore will be a quadrant, and conſequently E will coincide with P, being always in the plane of motion. The following properties are alſo derived from this conſtruction: 1ſt, The arc DE ſubtended by the obſerved objects is equal to the arc deſcribed by the index or moveable radius CP from O;

* Compare fig. 2.

differing in this from Mr. HADLEY's conſtruction, in which the angle obſerved is equal to double the angle deſcribed by the moveable radius from the initial point of the arc O. While therefore the moveable ſpeculum C is carried round by the radius CP in the plane of motion according to the new conſtruction juſt deſcribed, the image of E moves with an angular velocity juſt equal to that of the radius, the motion of the image being, according to Mr. HADLEY's invention, always greater than that of the radius in the proportion of 2 to 1. 2dly, The angles of incidence and reflection on both ſurfaces are conſtantly the ſame, being equal to 45°. 3dly, BI (art. 13.) being always leſs than 90°, obſervations by two reflections may be taken all round the circle, that is, angles of any magnitude may be meaſured by this conſtruction. It will not be difficult in practice to regulate the inclination of the plane reflectors to the plane of motion, with the other given quantities to their true magnitude. Let the reflectors B and C be brought parallel when the index or radius CP is directed to O, being the initial point of the arc OP: in order to examine whether the fixed plane of reflection BFD be coincident with the primitive ſecondary KO, it is only neceſſary to obſerve the angle ſubtended by two given objects when the index CP is on the different ſides of the initial point O: if the index be directed to unequal diſtances from that point at the times of obſervation, a correction is required (art. 11.). To examine whether the inclination of the reflectors to the plane of motion be exactly 45°, let the index CP be directed to 180°: if the inclination of the reflectors to each other be not then = 90°, a correction muſt be applied. It will be known whether the inclination of the reflectors to each other be = 90°, by obſerving the two oppoſite horizons at ſea, and at land by various obvious methods. Theſe examinations are wholly independent of the inclination of the teleſcope to the plane of motion, which is regulated to its true ſituation parallel to the plane OPA, by making the plane OPA fixed in regard to diſtant objects, and by obſerving if the images of objects E, ſeen after two reflections of the rays, deſcribe the arc of a great circle while C is carried round the plane of motion. Any three fixed objects, at a ſufficient diſtance, and ſituated in the ſame plane with the obſerver's eye, will be ſufficient for making this adjuſtment.

Fig. 9. 10. and 11. repreſent the progreſs of the rays, and the poſition of the reflectors according to this conſtruction. TC is a ray iſſuing from any object T in a direction parallel to the plane of the motion, and impinging on the ſpeculum C, which is inclined to that plane at an angle of 45°: from hence it is reflected in the direction CB perpendicular to the plane OCA, and being there reflected by the ſpeculum B proceeds in the given or conſtant direction BG parallel to the plane of motion OPA.

16. There is another conſtruction which follows from the general theory, the deſcription of which ſhould not be omitted. This will require ſome little explanation. As before (fig. 12.) let OPA repreſent the plane of motion, K its pole, FIM a parallel or leſſer circle projected on it, the diſtance of this parallel from the pole K being meaſured by the arc FK; let KO be the primitive ſecondary, and BFD the fixed plane of reflection on the ſpeculum B coincident with it. The other parts of the conſtruction * remaining, it has been demonſtrated (art. 10.), that the ſine of half the obſerved angle, that is, the ſine of half ED, is proportional to the ſine of IF, and the coſine of DL jointly. Every thing elſe being the ſame, it is manifeſt, that the ſine of half ED will be proportional to the ſine of IF: as therefore the arc KF, that is, the inclination of the reflecting planes to the plane of motion, is decreaſed,

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* Compare fig. 2. and 3.

the angle meaſured or arc ED will become ſmaller at the ſame time, becauſe FI decreaſes with KF, the angle IKF remaining. This property ſeems applicable to good purpoſe in meaſuring ſmall angles, not only from the great extent of ſcale, which is here obtained, but from various advantageous circumſtances, which will appear in the ſubſequent article, and from the computations annexed in thoſe which follow.

In this conſtruction the fixed plane of reflection is made coincident with the primitive ſecondary for various reaſons: there are only two poſitions of that fixed plane which admit of eaſy and exact adjuſtments; theſe are when the fixed plane of reflection is either perpendicular (art. 11.) to the primitive ſecondary or coincident with it. The latter poſition is preferred excluſive of the advantages it poſſeſſes in common with the other, becauſe it affords means for a very preciſe adjuſtment of the inclination of the reflecting planes to the plane of motion, that is, of the arc KF; for if the primitive ſecondary OKMD (fig. 12.) be produced, and in it DG be taken equal to four times KF, it is manifeſt, that when the perpendicular CI coincides with M, or, which is the ſame thing, when the radius CP is directed to 180°, the object E obſerved by two reflections will coincide with G, becauſe BF = FD and BM = MG by conſtruction. If then two given objects be obſerved when the index points to 180°, the inclination of the plane reflectors to the plane of motion will be one fourth part of the angle ſubtended by theſe objects.

Concerning the magnitude of the arc FB, being the meaſure of the angle of incidence on the fixed ſpeculum, and of KF = the inclination of the reflectors to the plane of motion, it will appear, by the computations*, that the ſmaller they are both taken, every thing elſe being the ſame, the more exact will be the reſult of the obſervation; but both are limited by circumſtances which ſhould next be deſcribed; theſe will be more obvious if an outline be annexed, repreſenting this particular caſe of the theory adapted to the menſuration of ſmall angles when reduced to practice. OAPC (fig. 13.) is the plane of motion, C the moveable ſpeculum carried round in the plane of motion by the radius CP; a ray coming from any diſtant object T impinges on the ſpeculum C, and being reflected in the direction CB, is there again reflected in the direction BG, paſſing along the axis of a teleſcope. A ray coming from another diſtant object S, inclined to the ray TC at a ſmall angle enters the teleſcope parallel to the direction of its axis, which is coincident with BG, and conſequently the images of the two objects S and T will be ſeen to coincide in the middle of the field of the teleſcope, the angle ſubtended by them being BGT, which muſt be determined by the ſubſequent computations.

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* Infra.

17. By this figure (fig. 13.), without further argument, it is plain, that the magnitude of the reflecting ſpeculum C limits the conſtant angle of incidence on B; for were that angle $= o$, the lines CB, BG, would coincide, by which means the ray BG and others adjacent to it would be intercepted from entering the teleſcope. The magnitude of the reflecting plane depends on the quantity of light required; if a circle of about 1 2 inches diameter be ſufficient, and the perpendicular diſtance of the reflecting planes be made equal to five inches, the leaſt angle of incidence, conſiſtent with theſe conditions, will be about 7°. It is however to be remembered, that the magnitude of the reflectors ſhould be adapted to the aperture of the teleſcope uſed in the obſervation. As the area of the ſpeculum increaſes, the light admitted into the ſame teleſcope decreaſes, and theſe areas ſhould be ſo proportioned as to afford equal quantities of light, ſo that the objects ſeen by two reflections, and by direct rays, may be nearly of equal brightneſs; but for the

ſake of conſtructing an example to this theory, the magnitude of the reflecting planes and the angle of incidence on the fixed ſpeculum B depending on it may be aſſumed of the value mentioned in this article.

18. The magnitude of the arc KF, or of the inclination of the reflecting planes to the plane of motion is limited by the angles which the obſerved objects ſubtend (art. 16.). Fig. 12. Becauſe, the greateſt angle obſervable will be meaſured by four times the arc KF, it follows, that the arc KF muſt not be leſs than one fourth part of the greateſt angle intended to be obſerved by this conſtruction; if the inclination denoted by the arc KF be fixed at 10′, four times that angle being 40′ will be greater than the apparent diameters of the ſun or any of the planets.

19. It remains to infer from the preceding conſtruction (fig. 2), the actual meaſure of the angle ſubtended by the objects obſerved. This muſt be effected by computation, which will not only ſerve as an illuſtration of the theory, but afford means of eſtimating and comparing the errors in the angle deduced, occaſioned by the unavoidable errors in obſervation and practical conſtruction; an examination extremely uſeful in aſtronomical ſubjects: next to removing errors entirely from obſervations, which is ſcarcely to be hoped for, the leſſening, circumſcribing, and reducing them within known limits is an object of principal conſequence.

20. The conſtruction of fig. 2. remaining, through the points F and I (fig. 14.) draw the great circle FI. Biſect FI in Q, and through the points K and Q draw the arc KQ, which will be perpendicular to IF. To determine by computation the arc ED which meaſures the angle ſubtended by the obſerved objects, three ſpherical triangles, KQF or KIF, IFB, and DBE, muſt be ſolved, for which the data are evidently ſufficient;

or the value of ED may be obtained from the ſolution of two triangles KQF and DFL, with the proportion demonſtrated in art. 9.

21. To proceed with the computation, through D draw the arc DL perpendicular to FI, and let the ſine of QKF $= p$, being the ſine of half the arc OP, the meaſure of IKF: put the ſine of KF $= s$, the ſine of DF $= m$, the ſine of DFK $= n$, radius $= 1$. In the right-angled ſpherical triangle KQF, the properties of ſpherics give this proportion: as radius to the ſine of KF ſo is the ſine of QKF to ſine of QF; wherefore ſin. QF $= sp$; coſ. QF $= \sqrt{1 - s^2p^2}$; and ſin. FI (FI being double to QF) $= 2sp \times \sqrt{1 - s^2p^2}$. Moreover, becauſe as rad. to coſ. QK ſo is coſ. QF to coſ. KF, we have coſ. KQ $= \frac{\sqrt{1-s^2}}{\sqrt{1-p^2s^2}}$; and ſin. KQ $= \frac{\sqrt{s^2-s^2p^2}}{\sqrt{1-s^2p^2}}$. And ſince as rad. : ſin. QFK ſo is ſin. KF to ſin. KQ; this proportion gives ſin. QFK $= \frac{\sqrt{1-p^2}}{\sqrt{1-s^2p^2}}$; and becauſe the ſine of the angle LFD is the ſine of the difference (or ſum) of the angles QFK, DFK, of which the ſines are, ſin. QFK $= \frac{\sqrt{1-p^2}}{\sqrt{1-s^2p^2}}$ juſt found, and ſin. DFK $= n$ by the data, we have from the rules of trigonometry,

$$\text{ſin. * DFL} = \frac{\sqrt{1-p^2} \times \sqrt{1-n^2} \mp \sqrt{p^2n^2 - n^2s^2p^2}}{\sqrt{1-s^2p^2}},$$

and ſince in the right-angled triangle LDF, as rad. : ſin. DF :: ſin. DFL : ſin. DL, and by the problem ſin. DF $= m$ it appears, that

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* If the points P and Q be on different * ſides of the point O as they are repreſented in the conſtruction, the laſt term will be affected with the ſign $-$: if P and Q be on the ſame ſide of O, the ſign of the laſt term will be $+$. It may be here obſerved, concerning the geometrical conſtruction (fig. 2. and 3.) that when P and Q are on different ſides of O, the angle obſerved ED will be greater than when thoſe points are on the ſame ſide of the initial point O, the arcs OP, Op, being equal.

* Compare fig. 2.

$$\text{ſin. DL} = \frac{\sqrt{1-p^2}\times\sqrt{m^2-m^2n^2}\mp\sqrt{p^2m^2n^2-p^2n^2m^2s^2}}{\sqrt{1-s^2p^2}}, \text{ and}$$

$$\overline{\text{ſin. DL}}^2 = \frac{m^2-m^2n^2-p^2m^2+2p^2m^2n^2-m^2n^2p^2s^2\mp 2m^2pn\times\sqrt{1-p^2}\times\sqrt{1-s^2}\times\sqrt{1-n^2}}{1-s^2p^2}$$

and the ſquare of the coſine of DL

$$= \frac{1-s^2p^2-m^2+m^2n^2+p^2m^2-2p^2m^2n^2+m^2n^2p^2s^2\pm 2m^2pn\times\sqrt{1-p^2}\times\sqrt{1-s^2}\times\sqrt{1-n^2}}{1-s^2p^2}$$

The ſine of IF was ſhewn to be $2sp\times\sqrt{1-s^2p^2}$, and its ſquare $= 4s^2p^2\times\overline{1-s^2p^2}$: moreover, it was demonſtrated in art. 9. that as $\overline{\text{rad.}}^2 : \overline{\text{coſ. DL}}^2 :: \overline{\text{ſin. IF}}^2 : \overline{\text{ſin. }\tfrac{1}{2}\text{ED}}^2$ which gives, by ſubſtituting the values of $\overline{\text{coſ. DL}}^2$ and $\overline{\text{ſin. IF}}^2$, and multiplying the $\overline{\text{coſ. DL}}^2$ into $\overline{\text{ſin. IF}}^2$, $\overline{\text{ſin. }\tfrac{1}{2}\text{ED}}^2 =$

$$4s^2p^2\times\overline{1-s^2p^2-m^2+m^2n^2+p^2m^2-2p^2m^2n^2+m^2n^2s^2p^2\pm 2m^2pn\times\sqrt{1-s^2}\times\sqrt{1-p^2}\times\sqrt{1-n^2}}$$

and the coſine of $\tfrac{1}{2}\text{ED}^2 =$

$$1-4s^2p^2\times\overline{1-s^2p^2-m^2+m^2n^2+p^2m^2-2p^2m^2n^2+m^2n^2s^2p^2\pm 2m^2pn\times\sqrt{1-s^2}\times\sqrt{1-p^2}\times\sqrt{1-n^2}}$$

finally the coſine of ED is therefore =

$$1-8s^2p^2\times\overline{1-s^2p^2-m^2+m^2n^2+p^2m^2-2p^2m^2n^2+m^2n^2s^2p^2\pm 2m^2pn\times\sqrt{1-s^2}\times\sqrt{1-p^2}\times\sqrt{1-n^2}}.$$

22. The particular caſes inferred from the geometrical conſtruction may be compared with this analytical value of the coſine of ED, or of the angle ſubtended by the obſerved objects. If $s=1$ and $n=1$, by ſubſtituting 1 for s and n in the expreſſion juſt found, we ſhall have the coſine of ED $= 1-8p^2+8p^4$, which is the coſine of an arc four times greater than that of which the ſine $=p$. This anſwers to the properties of HADLEY's inſtrument, in which KF or the inclination of the reflecting planes to the plane of motion is 90°, and its ſine $=1=s$: moreover, in HADLEY's inſtrument, the fixed plane of reflection at the unmoved ſpeculum is parallel to the plane of motion, and therefore perpendicular to any ſecondary of that plane; its inclination to any ſe-

condary therefore will be 90°, and the ſine of this inclination $= 1 = n$ by the problem. And ſince p is the ſign of half the inclination of the reflectors, the angle of which the coſine is $1 - 8p^2 + 8p^4$ will be twice the inclination of the reflecting planes, which is a property of HADLEY's inſtrument. In the analytical value of the coſine of ED, the laſt term is affected by two ſigns; theſe depend on the poſition of the ſecondary KP and the interſection Q in reſpect of the point O. If the ſecondary KP or the index CP be on the ſame ſide of O with the interſection Q (fig. 2.), the ſign of the laſt term is negative: if CP and Q be on oppoſite ſides of O, the ſign of the laſt term will be poſitive; and when $DFK = o$ or 180°, the whole term vaniſhes, becauſe in that caſe $n = o$. Alſo, if $m = 0$, $n = 1$, $s = 1$, or if $p = 1$, the laſt term vaniſhes. When $m = s = \frac{1}{\sqrt{2}}$, $KF = 45°$: in this caſe, if $n = o$ the conſtruction will be that deſcribed in art. 15. and the coſine of the obſerved angle ED will equal $1 - 2p^2$, the other terms vaniſhing: and becauſe $1 - 2p^2$ is the coſine of an arc double to that of which the ſine $= p$, it follows, that the angle obſerved will be equal to the arc deſcribed by the index from o, of which the ſine of one half is by the problem $= p$. In every caſe, when $n = o$, that is, when the fixed plane of reflection at the unmoved ſpeculum coincides with the primitive ſecondary KO (fig. 2. and 12.), the coſine of $ED = 1 - 8s^2p^2 \times \overline{1 - s^2p^2 - m^2 + p^2m^2}$.

23. The ſine of ED will be neceſſary (art. 27.) to aſcertain the variation of ED from the truth occaſioned by errors in the data; to obtain ſin. ED let

$1 - s^2p^2 - m^2 + m^2n^2 + p^2m^2 - 2p^2m^2n^2 + m^2n^2s^2p^2 \pm 2m^2np \times \sqrt{1-s^2} \times \sqrt{1-p^2} \times \sqrt{1-n^2}$ $= d$: then (art. 21.) from the value of $\overline{\text{coſ.}\ \tfrac{1}{2}ED}^2$ we have ſin. $ED = 4sp \times \sqrt{d} \times \sqrt{1 - 4s^2p^2d}$. When s is very ſmall, and $n=o$, $d=1-m^2+m^2p^2$ nearly, which gives

$$\text{ſin. ED}=4sp\times\sqrt{1-m^2+m^2p^2}\text{ nearly.}$$

24. The coſine of the obſerved angle repreſented by ED (fig. 2. and 14.) in the conſtruction, being computed from the four given quantities p, s, m, and n, if either of theſe ſhould deviate from its true value, the angle deduced will be erroneous: and from the general expreſſion for the coſine of ED, an eſtimation of this error will be obtained. In the inveſtigation, however, it muſt be obſerved, that although the ſmall increments or decrements of arcs or ſines are aſſumed proportional to the fluxions of theſe quantities, which is ſtrictly true only in the naſcent ſtate of the increments or decrements, yet when the given variations are in a practical ſenſe very ſmall, the eſtimation of correſponding variations will be in general ſufficiently exact for practical purpoſes.

25. Small increments and decrements, that is, ſmall variations, being aſſumed proportional to the fluxions of arcs and of their ſines and coſines, if the variation of the ſine or coſine of any given arc be known, the cotemporary variation of the arc will be for the moſt part inferred from the following proportions: as ſin. : rad. :: $-\overset{\cdot}{\overline{\text{coſ.}}}$: $\overset{\cdot}{\overline{\text{arc}}}$; and as coſ. : rad. :: $\overset{\cdot}{\overline{\text{ſin.}}}$: $\overset{\cdot}{\overline{\text{arc.}}}$ But theſe proportions muſt be uſed under reſtrictions very neceſſary to be inſerted in this place, being true when applied to the intermediate parts of the quadrant only and failing at the extremities; for example, at the very beginning of the quadrant, or at the very end of the ſemi-circle, the variation of the coſine is the verſed ſine of the arcs increment or decrement, which gives the proportion as ſin. : 2 × rad. :: $-\overset{\cdot\cdot}{\overline{\text{coſ.}}}$: $\overset{\cdot\cdot}{\overline{\text{arc}}}$, being wholly different from the former: in like manner, at the very extremity of the quadrant,

drant, the increment of the ſine becomes the verſed ſine of the arcs laſt increment, which gives this proportion: as coſ. : 2 × rad. :: $\dot{\overline{\text{ſin.}}}$: $\dot{\overline{\text{arc.}}}$ And ſince in this caſe $\dot{\overline{\text{arc}}}$ = coſine, we ſhall have, $\dot{\overline{\text{arc}}} = \sqrt{2 \times \dot{\overline{\text{ſin.}}}}$ radius being = 1. In the other parts of the quadrant which are not very near its extremity, $\dot{\overline{\text{arc}}} = \frac{\dot{\overline{\text{ſin.}}}}{\text{coſ.}}$; having given, therefore, the variation of the ſine or coſine of any arc, the ſine or coſine being known, the cotemporary variation of the arc itſelf may be obtained, when it is either at the very extremities of the quadrant, or at ſome diſtance from thoſe extremities. The difficulty lies in aſcertaining in what part of the quadrant the value of the $\dot{\overline{\text{arc}}} = \frac{\dot{\overline{\text{ſin.}}}}{\text{coſ.}} = \frac{-\dot{\overline{\text{coſ.}}}}{\text{ſin.}}$ begin to fail, and the value expreſſed by $\dot{\overline{\text{arc}}} = \sqrt{2 \times \dot{\overline{\text{ſin.}}}}$ or $-\sqrt{2 \times \dot{\overline{\text{coſ.}}}}$ to take place. This leads to a general propoſition comprehending both theſe values for the arc's variation, extended to every part of the quadrant.

The propoſition is this: the difference of the coſines is to the chord of the difference of any two arcs, as the ſine of an arithmetical mean between them to radius; and the difference of the ſines is to the chord of the difference, as the coſine of the ſame arithmetical mean to radius. Let AB, AF (fig. 15.) be the given arcs; BF their difference; BL, FH, the ſines; CL, CH, the coſines of the arcs AB, AF, reſpectively; join CA, CB, CF, and FB; FB will be the chord of the difference of the arcs AF, AB. Through B draw BG parallel to CA; then HL = BG will be the difference of the coſines, and FG the difference of the ſines. Biſect FB in D, ſo ſhall DA be an arithmetical mean between the arcs FA, BA; join DC, which will interſect FB at right angles in E: through D and E draw DK, EI, perpendicular to CA: DK will be the ſine, and CK the coſine of the mean arithmetical DA: the ſimilar triangles CEI, CDK, FGB, give the following proportions:

HL or GB : FB :: DK : DC, and

GF : FB :: CK : DC, which was the propoſition to be demonſtrated *.

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* When FB (fig. 15) is ſo ſmall in compariſon of FA, that FG ſhall be evaneſcent in compariſon of FH, FH and BL will be in the ratio of equality, and conſequently the ratio FH : FC equal to the ratio BL : BC, or to the ratio DK : DC; for this reaſon, and becauſe it has been proved, that as HL : FB :: DK : DC, it follows, that as HL : FB :: FH or BL : BC, that is, as the variation of the coſine is to cotemporary variation of the arc, ſo is the ſine of the varying arc to radius; and, for ſimilar reaſons, as the variation of the ſine is to the cotemporary variation of the arc, ſo is the coſine to radius.

If BA be ſo diminiſhed that FG ſhall bear a finite proportion to FH, and too great to be neglected, BL will not be either to FH or to DK in a ratio of equality: conſequently, FH or BL muſt no longer be ſubſtituted for DK: as BA becomes leſs, FB being ſtill ſuppoſed evaneſcent, DK approximates to the ſine of ½FB to which it is ultimately equal when B and F arecoinciding with A (fig. 16.). In which caſe the proportion will become as HL or HA : FB or FA :: ½FA : to CA, that is, as the verſed ſine of FA is to the arc FA ſo is half the arc FA to radius, or ſo is the arc FA to diameter.

The propoſitions which have been demonſtrated, comprehend the variation of the arc expreſſed in terms of the cotemporary variation of the ſine or coſine in every part of the quadrant without limitation, it being only allowed to ſubſtitute the arc FB inſtead of its chord, theſe quantities approximating the more nearly to equality as FB is ſmaller, and being ultimately equal in their evaneſcent ſtate. Moreover, it will be eaſy from what has preceded to conſtruct a plane right-lined triangle, which ſhall be ſimilar to the mixtilinear triangle contained under an arc, its ſine and verſed ſine when they are diminiſhed ſine limite. Let FA (fig. 16.) be any arc, FA the chord, FH the ſine, CH the coſine of the arc FA. Biſect FA in D, join CD, and draw the right ſine DK: then will the plane right-lined triangle KDC continually approximate to ſimilarity with the mixtilinear triangle FDAH as FA becomes ſmaller, and the two triangles will be ultimately ſimilar when FA is vaniſhing.

From theſe geometrical proportions, having given any arc and the variation of its ſine or coſine, the cotemporary variation of the arc may be eſtimated by computation in general for any part of the quadrant. Let the ſine of any arc be s, the coſine $= c$, the chord of the arc's variation $= x$, the given variation of the coſine $= d$, or the given variation of the ſine $= b$, radius $= 1$; then if the coſine of the arc increaſes by the difference d, the chord of the cotemporary decreaſe of the arc, or

$$-x = \sqrt{2s^2 - 2dc \mp \sqrt{\overline{2s^2 - 2dc}^2 - 4d^2}}$$

and if the ſine of the given arc increaſes by the difference b

$$+x = \sqrt{2c^2 - 2bs \mp \sqrt{\overline{2c^2 - 2bs}^2 - 4b^2}},$$

which are the mathematically true values of the chord FB, and will approximate to the magnitude of the arc FB as that arc is continually diminiſhed. The following expreſſions for the chord of the variation x are more compendious, and will be ſufficiently near the truth when FB is very ſmall.

$$-x = \frac{s}{c} - \sqrt{\frac{s^2}{c^2} - \frac{2d}{c}}$$

$$+x = \frac{c}{s} - \sqrt{\frac{c^2}{s^2} - \frac{2b}{s}}.$$

In theſe four expreſſions it muſt be obſerved, that the ſine and coſine are ſuppoſed to vary by increaſe: ſhould the variation be a decrement, the ſign of x and of b or d muſt be changed.

26. Let the quantities p, s, m, n, vary by ſmall increments $\dot{p}$, $\dot{s}$, $\dot{m}$, $\ddot{n}$, reſpectively, then to obtain the cotemporary variation of coſ. ED, becauſe (art. 21.)

$$\text{Coſ. ED} = 1 - 8s^2p^2 \times \overline{1 - s^2p^2 - m^2 + m^2n^2 + m^2p^2 - 2p^2m^2n^2 + m^2n^2s^2p^2 \pm 2m^2np \times \sqrt{1 - p^2} \times \sqrt{1 - n^2} \times \sqrt{1 - s^2}},$$

by taking the fluxion of the equation we have

$$\overline{\text{Coſ.}\,\dot{ED}} = -16s^2p\dot{p}\times\overline{1-2s^2p^2-m^2+n^2m+2m^2p^2-4m^2n^2p^2+2n^2m^2s^2p^2\pm\frac{m^2np\times\sqrt{\overline{1-n^2}}\times\sqrt{\overline{1-s^2}}\times\overline{3-4p^2}}{\sqrt{\overline{1-p^2}}}}$$

$$-16p^2s\dot{s}\times\overline{1-2s^2p^2-m^2+n\,m^2+p^2m^2-2p^2m^2n^2+2p^2m^2n^2s^2\pm\frac{m^2np\times\sqrt{\overline{1-n^2}}\times\sqrt{\overline{1-p^2}}\times\overline{2-3s^2}}{\sqrt{\overline{1-s^2}}}}$$

$$+16s^2p^2m\dot{m}\times\overline{1-n^2-p^2+2p^2n^2-s^2p^2n^2\mp2pn\times\sqrt{\overline{1-n^2}}\times\sqrt{\overline{1-s^2}}\times\sqrt{\overline{1-p^2}}}$$

$$+16s^2p^2m^2\dot{n}\times\overline{-n+2p^2n-s^2p^2n\mp\frac{\sqrt{\overline{1-s^2}}\times\sqrt{\overline{1-p^2}}\times p\times\overline{1-2n^2}}{\sqrt{\overline{1-n^2}}}}.$$

27. This value of $\overline{\text{coſ.}\,\dot{\text{ED}}}$ is expreſſed in terms of the variation of the ſines of the given quantities: if it be neceſſary to expreſs $\overline{\text{coſ.}\,\dot{\text{ED}}}$ in terms of the variation of the arcs themſelves, it muſt firſt be conſidered to what part of the quadrant they belong: for example, if s be a ſine of an arc b not very near the extremity of the quadrant, and the variation be $\dot{s}$, the cotemporary variation of the arc $\dot{b}$ will be $\frac{\dot{s}}{\sqrt{\overline{1-s^2}}}$; but if the variable arc be nearly $=90°$, and becomes exactly equal to it ultimately having varied by a ſmall arc $\dot{b}$ of which the verſed ſine $=v$; then will $-\dot{s}=$ the verſed ſine of $\dot{b}$ and $-\dot{b}=\sqrt{2v}$. Laſtly, if the variable angle approximates to 90°, but is not equal to it, and the variation of its ſine ſhould be $=\dot{s}$, the cotemporary variation of the arc muſt be obtained from the general theorem in art 25. When either of the two latter caſes happen, the variation of the arc muſt be determined for each particular caſe; but it will be neceſſary to give a general expreſſion for $\overline{\text{coſ.}\,\dot{\text{ED}}}$ in terms of the variations of the given arcs, of which p, s, m, n, are the reſpective ſines when theſe arcs are at ſome diſtance from 90°; this is contained in the next article.

28. Let the angle QKF $= a$ (fig. 14.); the arc KF $= b$; the arc DF $= c$, and the angle DFK $= d$; their reſpective increments being $\dot{a}$, $\dot{b}$, $\dot{c}$, and $\dot{d}$, their ſines p, s, m, and n, and the contemporary increments of their ſines $\dot{p}$, $\dot{s}$, $\dot{m}$, and $\dot{n}$: from the proportion contained in art. 24. we ſhall have $\dot{p} = \dot{a} \times \sqrt{1-p^2}$, $\dot{s} = \dot{b} \times \sqrt{1-s^2}$, $\dot{m} = \dot{c} \times \sqrt{1-m^2}$, and $\dot{n} = \dot{d} \times \sqrt{1-n^2}$, which being ſubſtituted in the value of $\overline{\text{coſ.}\,\dot{\text{ED}}}$ laſt found will give

$$\overline{\text{Coſ.}\,\dot{\text{ED}}} = -16s^2 \times \sqrt{1-p^2} \times p\dot{a} \times \overline{1-2s^2p^2-m^2+n^2m^2+2m^2p^2-4m^2n^2p^2+2s^2m^2n^2p^2 \pm \frac{m^2np \times \sqrt{1-s^2} \times \sqrt{1-n^2} \times \overline{3-4p^2}}{\sqrt{1-p^2}}}$$

$$-16p^2 \times \sqrt{1-s^2} \times s\dot{b} \times \overline{1-2s^2p^2-m^2+n^2m^2+p^2m^2-2p^2m^2n^2+2s^2m^2n^2p^2 \pm \frac{m^2np \times \sqrt{1-n^2} \times \sqrt{1-p^2} \times \overline{2-3s^2}}{\sqrt{1-s^2}}}$$

$$+16s^2p^2 \times \sqrt{1-m^2}\,m\dot{c} \times \overline{1-n^2-p^2+2p^2n^2-s^2p^2n^2 \mp 2pn \times \sqrt{1-n^2} \times \sqrt{1-s^2} \times \sqrt{1-p^2}}$$

$$+16s^2p^2m^2 \times \sqrt{1-n^2} \times \dot{d} \times \overline{-n+2p^2n-s^2p^2n \mp \frac{p \times \sqrt{1-s^2} \times \sqrt{1-p^2} \times \overline{1-2n^2}}{\sqrt{1-n^2}}}.$$

This quantity (art. 23.) being divided by the ſine of the obſerved angle, the variation of that angle or $\overline{\dot{\text{ED}}}$ will be the quotient.

29. In the expreſſion for $\overline{\text{coſ.}\,\dot{\text{ED}}}$ contained in art. 26. the variations $\dot{p}$, $\dot{s}$, $\dot{m}$, and $\dot{n}$, are arbitrary, as are $\dot{a}$, $\dot{b}$, $\dot{c}$, and $\dot{d}$, in the laſt article. If a condition be annexed to the variation of any of them, two or more may become dependant on each other; and their relation muſt be determined by the nature of the caſe. Moreover, if one or more of the given arcs and their ſines ſhould be correct, the variations correſponding and all the terms multiplied into them will vaniſh. To give an example of the uſe of theſe expreſſions before they are applied to the immediate purpoſe of examining the new conſtructions

deſcribed in art. 15. and 16. let it be required to aſſign what error is occaſioned in obſerving a given angle with a HADLEY's ſectant, in which the teleſcope is parallel to the plane of motion, but the two reflectors deviate from their perpendicular to that plane by a ſmall angle $\dot{b}$. Suppoſe the error of half the arc pointed to by the index to be $\dot{a}$, and conſequently the error of the ſine of half that * arc $= \dot{a} \times \sqrt{1 - p^2} = \dot{p}$: in this caſe, becauſe the inclination of the reflectors to the plane of motion is nearly $= 90°$, the variation of the ſine will be equal to the verſed ſine of the ſmall arc $\dot{b}$, by which the inclination deviates from 90°; let v be the verſed ſine of $\dot{b}$, then will $-\dot{s} = v$ (s varying by a decrement of v). Moreover, becauſe a condition is annexed, which is, that the line of obſervation is parallel to the plane of motion, the variations $\dot{s}$, $\dot{m}$, and $\dot{n}$, will be dependent on each other. To inveſtigate their relation let FO $= \dot{b}$ (fig. 7.) be the ſmall arc which meaſures the deviation of the reflectors from the perpendicular to the plane of motion: then, becauſe ſin. DO $= m$, and ſin. DOF $= n = 1$ by the problem, when F from having been coincident with O has moved through the arc OF, it is plain, that $\dot{n} =$ ſin. DFO − ſin. DOF $=$ ($\dot{n}$ being a decrement); but, by the † properties of ſpherics, coſ. DFO $= \frac{\sqrt{2v} \times \text{coſ. DF}}{\text{ſin. DF}} = \frac{\sqrt{2v} \times \sqrt{1 - m^2}}{m}$: and FO being very ſmall, the

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* p here, as in the general ſolution, denotes the ſine of half the arc to which the index on the plane of motion is directed, that is, $p =$ the ſine of one-fourth of the angle obſerved in Mr. HADLEY's conſtruction.

† Fig. 7. as rad. : cotang. DF :: tang. FO : coſ. DFO, that is, FO being very ſmall, and therefore $\overline{\text{FO}}^2 = 2 \times$ verſed ſine of FO, as rad. : cotang. DF :: $\sqrt{2v}$: coſ. DFO : by the problem ſin. DF $= m$, and coſ. DF $= \sqrt{1 - m^2}$, wherefore cot. DF $= \frac{\sqrt{1 - m^2}}{m}$, which gives coſ. DFO $= \sqrt{2v} \times \frac{\sqrt{1 - m^2}}{m}$.

ſine of DFO $= 1 - \frac{v \times \overline{1-m^2}}{m^2}$, from which $1 =$ ſin. DOF being ſubtracted leaves $\dot{n} = \frac{-v \times \overline{1-m^2}}{m^2}$; or becauſe $\dot{s} = -v$, $\dot{n} = \frac{+\dot{s} \times \overline{1-m^2}}{m^2}$. Moreover, the* quantity $\frac{1-s^2}{1-n^2}$ in the naſcent ſtate of $\overline{1-s^2}$ and $\overline{1-n^2} = \frac{m^2}{1-m^2}$: and $\sqrt{\frac{1-s^2}{1-n^2}} = \frac{m}{\sqrt{1-m^2}}$. Making therefore in the general expreſſion contained in art. 26. $\frac{-v \times \overline{1-m^2}}{m^2} = \dot{n}$, $-v = \dot{s}$, $\dot{a} \times \sqrt{1-p^2} = \dot{p}$, and $\frac{m}{\sqrt{1-m^2}} = \sqrt{\frac{1-s^2}{1-n^2}}$, $n = s = 1$, we ſhall have $\overline{\text{Coſ.}\,\dot{ED}} = -16p\dot{a} \times \sqrt{\overline{1-p^2}} \times \overline{1-2p^2} + 16p^2v \times \overline{1-2p^2+p^2m^2 \mp mp \times \sqrt{\overline{1-m^2}} \times \sqrt{\overline{1-p^2}}}$ $+ 16p^2v \times \overline{1-p^2-m^2+m^2p^2 \mp mp \times \sqrt{\overline{1-m^2}} \times \sqrt{\overline{1-p^2}}} = -16p\dot{a} \times \sqrt{\overline{1-p^2}} \times \overline{1-2p^2} +$ $16p^2v \times \overline{2-3p^2-m^2+2p^2m^2 \mp 2mp \times \sqrt{\overline{1-m^2}} \times \sqrt{\overline{1-p^2}}}$: and becauſe the ſine of the obſerved angle is $4p \times \sqrt{\overline{1-p^2}} \times \overline{1-2p^2}$, the error of the obſervation itſelf, that is,

$$\dot{\overline{ED}} = 4\dot{a} - \frac{4pv \times \overline{2-3p^2-m^2+2p^2m^2 \mp 2mp \times \sqrt{\overline{1-m^2}} \times \sqrt{\overline{1-p^2}}}}{\sqrt{\overline{1-p^2}} \times \overline{1-2p^2}}.$$

In this example the poſition of the teleſcope has been ſuppoſed exactly in the plane of motion; ſhould it be inclined to that plane at a ſmall angle, of which the verſed ſine $= v$, the poſition of the reflectors and the arc pointed to by the index being correct, the general value of $\overline{\text{coſ.}\,\dot{ED}}$ will give the error of the obſervation, or $\dot{\overline{ED}} = \frac{-4pv \times \sqrt{\overline{1-p^2}}}{1-2p^2}$ †.

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* The naſcent value of $\frac{1-s^2}{1-n^2} = \frac{-2s\dot{s}}{-2n\dot{n}}$; but $\dot{n} = \frac{-v \times \overline{1-m^2}}{m^2} = \frac{+\dot{s} \times \overline{1-m^2}}{m^2}$: wherefore $\frac{1-s^2}{1-n^2} = \frac{m^2}{1-m^2}$, when s and n are nearly $= 1$.

† When the poſition of the teleſcope only is erroneous, the points F and O coincide:

30. To examine in what degree an obſervation taken by the new conſtruction deſcribed in art. 15. is affected by known errors in the given quantities, let the reflectors B and C deviate by exceſs from their true angle of inclination to the plane of motion by a ſmall angle $+\dot{b}$: let the angle of incidence on the fixed ſpeculum be too great by the increment $\dot{c}$: let the fixed plane of reflection deviate from the ſecondary KO with which it ſhould coincide by a ſmall angle $\dot{d}$; and laſtly, let the error of the arc pointed to by the index be $= 2\dot{a}$; then theſe variations are arbitrary, no condition being annexed. Moreover, by the conſtruction $m = s = \frac{1}{\sqrt{2}}$, and $n = 0$, which values being ſubſtituted in the general expreſſion contained in art. 28. we ſhall have.

$$\overline{\text{Coſ.}\dot{\text{ED}}} = -4\dot{a}p \times \sqrt{\overline{1-p^2}} - 4p^2\dot{b} \times \overline{1-p^2} + 4p^2\dot{c} \times \overline{1-p^2} \mp 2\sqrt{2}p^3\dot{d} \times \sqrt{\overline{1-p^2}};$$

and becauſe the ſine of the angle meaſured $= 2p \times \sqrt{\overline{1-p^2}}$, the error of the obſervation required, or

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coincide (fig. 7.) let the inclination of the teleſcope to the plane of motion with which it ſhould coincide, be meaſured by the ſmall arc D*d*; then the correſponding variation of the angle DOK will be DO*d*. Let D*d* $= e$, and its verſed ſine $= v$; ſince the ſine of DO $= m$, and the ſine of DOK $= 1 = n$ by the problem, $\dot{n} =$ the verſed ſine of DO*d*; but DO*d* $= \frac{e}{m}$, and the verſed ſine of DO*d* $= \frac{e^2}{2m^2} = \frac{v}{m^2}$: wherefore $\dot{n} = \frac{-v}{m^2}$. This being premiſed, it appears from art. 26. when $\dot{p}$, and $\dot{s}$, are $= 0$, that $\overline{\text{coſ.}\ \dot{\text{ED}}} = +16s^2p^2m^2\dot{n} \times \overline{-n+2p^2n-s^2p^2n}, \pm \frac{\sqrt{\overline{1-s^2}} \times \sqrt{\overline{1-p^2}} \times p \times \overline{1-2n^2}}{\sqrt{\overline{1-n^2}}}$

$$+16s^2p^2m\dot{m} \times \overline{1-n^2-p^2+2p^2n^2-s^2p^2n^2 \mp 2pn \times \sqrt{1-n^2} \times \sqrt{1-s^2} \times \sqrt{1-p^2}}$$

in which quantity, ſubſtituting 1 for s, 1 for n, and $\frac{-v}{m^2}$ for $\dot{n}$, we ſhall have $\overline{\text{coſ.}\ \dot{\text{ED}}} = +16p^2v \times \overline{1-p^2}$, which being divided by the ſine of the obſerved angle $= 4p \times \sqrt{\overline{1-p^2}} \times \overline{1-p}$, the quotient will be the variation of that angle or $\dot{\overline{\text{ED}}} = \frac{-4vp \times \sqrt{\overline{1-p^2}}}{1-2p^2}$.

$$\overline{\dot{ED}} = 2\dot{a} + 2p\dot{b} \times \sqrt{1-p^2} - 2p\dot{c} \times \sqrt{1-p^2} \pm \sqrt{2p^2}\,\dot{d}.$$

It appears from the firſt term $2\dot{a}$, that an error in the arc pointed to by the index, cauſes an equal error in the obſerved angle; whereas a double error is cauſed by it in HADLEY's ſextant, which gives the new conſtruction conſiderable advantages: to counter-balance theſe the errors in HADLEY's conſtruction, cauſed by a wrong poſition of the reflecting planes, &c. are almoſt evaneſcent; whereas the three laſt terms in the value of $\overline{\dot{ED}}$ juſt found, may become conſiderable, unleſs great care be taken in making the adjuſtments: the ſeparation alſo of the images, when in contact, cauſed by any unſteadineſs of the inſtrument will be greater than in Mr. HADLEY's conſtruction *; but

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* The variation of the obſerved angle $\overline{\dot{ED}}$ will ſhew how much the images ſeen in contact in the field of the teleſcope will appear to divaricate on any motion of the entire conſtruction. For example, while the images of the obſerved objects are coincident in the field of the teleſcope, ſuppoſe that Mr. HADLEY's inſtrument were turned round in its own plane through a ſmall angle: here s, n, and p, not being affected by this motion, it follows, that $\dot{s}$, $\dot{n}$, and $\dot{p} = 0$, $m =$ the ſine of the angle of incidence on the fixed ſpeculum, being the only quantity which ſuffers alteration: let its variation $= \dot{m}$, which will give from art. 26. the correſponding variation in the coſine of the obſerved angle, or

$$\overline{\dot{\text{Coſ.}}\ \overline{ED}} = +16s^2p^2m\dot{m} \times \overline{1-n^2-p^2+2p^2n^2-s^2p^2n^2 \mp 2pn \times \sqrt{1-n^2} \times \sqrt{1-s^2} \times \sqrt{1-p^2}}$$

$=0$, becauſe $n = s = 1$. Wherefore any motion of the images in the plane of the inſtrument will not cauſe the leaſt ſeparation of them. Now ſuppoſe the whole to be turned on an axis ſituated in the plane of motion, and perpendicular to the teleſcope's axis: if the angular motion be meaſured by an arc of which the verſed ſine $= v$, the points in contact will be ſeparated through an angle $= \frac{4vp \times \sqrt{1-p^2}}{1-2p^2}$, p being the ſine of one quarter of the angle obſerved; but the quantity v being very ſmall, when the angular motion does not exceed 30′, the divarication of the images will be inconſiderable. All oblique motions of the teleſcope's axis, and conſequently of the image ſeen by direct rays, may be reſolved into thoſe that have but in meaſuring the ſmaller angles, this ſeparation of the images, as well as the errors expreſſed by the three laſt terms will be greatly diminiſhed while that which is denoted by $2\dot{a}$ contained in the firſt term is not increaſed. On the whole, from the properties which have been demonſtrated to belong to this conſtruction deſcribed in art. 15. it may ſeem worthy of attention in practice, for ſome aſtronomical as well as other uſes.

31. By the ſame way of examination it may be judged, whether the method of obſerving by two reflections from plane ſurfaces be applicable to the menſuration of ſmall angles, according to the conſtruction deſcribed in art. 16. Let the errors of the four given quantities (as in the laſt article) be $2\dot{a} =$ the error of the arc

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have been conſidered, which are perpendicular to each other: and from hence the reaſon appears, why the motion of a ſhip at ſea does not much diſturb the obſervation of angles by Mr. HADLEY's inſtrument.

The new conſtruction deſcribed in art. 15. is not ſo well adapted for obſervation where it cannot be ſteadily fixed. When the images are in contact, if the inſtrument be turned in its own plane through a ſmall angle e'' the ſeparation of the images will be $= 2e''p^2$ (becauſe $d'' = \sqrt{2}e''$, vid. p. 426.) p ſignifying the ſine of half the obſerved angle: this it is evident will moſt affect the obſervation of the larger angles; but in meaſuring thoſe that are ſmall, the divarication will become inconſiderable. Moreover, if the angular motion of the inſtrument be e'', when it turns round an axis in the plane of motion, and perpendicular to the teleſcope's axis, the ſeparation of the images will be $= 2e''p \times \sqrt{1-p^2}$, p. 426. which it is plain will moſt diſturb the obſervations of angles about 90°, but will ſcarcely alter the contact of the images, when the angles meaſured are very ſmall, or near 180°.

The objects obſerved and their images are here underſtood to be phyſical points: thus, when the two images of the ſun are ſeen by direct and reflected rays, and the limbs appear preciſely in contact, if by any motion of the inſtrument the contact is diſturbed, the points which before touched, being the obſerved objects, are ſaid to be ſeparated, whether the centres of the ſolar images approach or recede from each other, the ſeparation being eſtimated in the direction of an arc which paſſes through the centers of the two ſolar images.

Experience muſt determine in what degree this ſeparation of the images will diſturb obſervations taken at ſea with the new conſtruction.

pointed to by the index; $\dot{b}$ = the error of the inclination of the reflectors to the plane of motion; $\dot{c}$ = the error in the angle of incidence on the fixed ſpeculum; and $\dot{d}$ = the inclination of the fixed plane of reflection to the primitive ſecondary with which it ſhould coincide. Referring to the general value of $\overline{\text{coſ. }\dot{E}D}$, (art. 28.) and making $n=o$ we ſhall have

$$\overline{\text{Coſ. }\dot{E}D} = -16\dot{a}\times\sqrt{\overline{1-p^2}}\,ps^2\times\overline{1-2s^2p^2-m^2+2m^2p^2}-16p^2s\dot{b}\times\sqrt{\overline{1-s^2}}\times\overline{1-2p^2s^2-m^2+m^2p^2}$$
$$+16s^2p^2\dot{c}m\times\sqrt{\overline{1-m^2}\times\overline{1-p^2}}\mp16m^2p^3s^2\dot{d}\times\sqrt{\overline{1-s^2}}\times\sqrt{\overline{1-p^2}},$$

and becauſe s being very ſmall, the ſine of ED (art 23.) approximates to $4sp\times\sqrt{\overline{1-m^2+p^2m^2}}$, the error of the obſervation itſelf, or

$$\overline{\dot{E}D} = \frac{4\dot{a}s\times\sqrt{\overline{1-p^2}}\times\overline{1-2p^2s^2-m^2+2p^2m^2}}{\sqrt{\overline{1-m^2+p^2m^2}}}$$
$$+\frac{4p\dot{b}\times\sqrt{\overline{1-s^2}}\times\overline{1-2p^2s^2-m^2+m^2p^2}}{\sqrt{\overline{1-m^2+p^2m^2}}}$$
$$-\frac{4spm\dot{c}\times\sqrt{\overline{1-m^2}\times\overline{1-p^2}}}{\sqrt{\overline{1-m^2+p^2m^2}}}$$
$$\pm\frac{4m^2p^2s\dot{d}\times\sqrt{\overline{1-s^2}}\times\sqrt{\overline{1-p^2}}}{\sqrt{\overline{1-m^2+p^2m^2}}}.$$

The firſt term of this expreſſion gives the relation between any ſmall variation in the arc pointed to by the index, and the correſponding alteration in the angle obſerved; if therefore the variation on the divided arc be any ſmall angle $\pm 2\dot{a}$, + or − the firſt term will expreſs the variation by which the obſerved angle is increaſed or diminiſhed. According to the magnitude of m and s aſſumed for this conſtruction m being the ſine of 7°, and s = the ſine of 10′, it appears, that at the very beginning of the ſcale one ſecond of a degree in the angle obſerved correſponds to ſomewhat leſs than three minutes on the divided arc OP; that is, when $2\dot{a}$ = about 173″, $\overline{\dot{E}D} = 1''$, $\dot{c}$, $\dot{b}$, and $\dot{d}$, not being here conſidered. When $p=\frac{1}{2}$, the index then pointing to 60°, one ſecond in the obſerved angle correſponds to about 199″ in the divided arc OP: when p is nearly $= 1$, the index being then directed to almoſt 180°, it muſt deſcribe above 2 degrees to make an alteration of 1″ in the obſerved angle. The ſecond term expreſſes the variation in the obſervation occaſioned by an error $\dot{b}$ in adjuſting the inclination of the reflecting planes to the plane of motion; but $\dot{b}$ (art. 18.) cannot exceed ¼ of the leaſt angle viſible in the teleſcope, conſequently the utmoſt value of the ſecond term cannot be ſo great as that leaſt angle, being at its limit when $p = 1$: it is manifeſt when p is ſmall, that the ſecond term is ſo much diminiſhed, as to be in a phyſical ſenſe evaneſcent. The ſame may be ſaid of the fourth term, containing the error of the optical adjuſtment $\dot{d}$, which beſides is multiplied into s the ſine of 10′. The third term is occaſioned by the error $\dot{c}$, for which, conſiderable latitude muſt be allowed, ſuppoſe 3′: to eſtimate the effect of this error on the obſervation, let a caſe be aſſumed: let the index be directed to 90° when an obſervation is taken for determining the angle ſubtended between two objects: then will $p = \frac{1}{\sqrt{2}}$; by ſubſtituting $\frac{1}{\sqrt{2}}$ for p, the ſine of 7° for m, the ſine of 10′ for s, and 180″ for $\dot{c}$ in the third term, we ſhall have, by computation, the value of that term, or the error in the obſervation occaſioned by this deviation of the angle of incidence from its true magnitude $= .''090$ not the tenth part of a ſecond. This is rather an unfavourable caſe, the variation being not much leſs than at its maximum when $p = \frac{1}{\sqrt{2}}$: if p is ſmall, or nearly $= 1$, the * variation will be wholly inſenſible.

32.

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* The variation is a maximum when $p =$ the ſine of 35° 12′; and conſequently the arc pointed to by the index $= 70°\ 24'$. Subſtituting therefore the ſine of 35° 12′

for

32. From contemplating the value of the error = $\overline{ED}$, as expreſſed by the four terms juſt referred to, ſome further obſervations are ſuggeſted. It appears, that ſetting aſide the error of the ſcale expreſſed by the firſt term (the angles meaſured being here ſuppoſed leſs than 22′ 56″ 33‴) all the others become ſmaller as the quantity p, and conſequently the angle meaſured is diminiſhed, p being multiplied into each of the three laſt terms. This is a material circumſtance: for the ſame given error would affect the menſuration of the ſmaller angles in a greater proportion than the larger. Moreover, if $p = 1$, or approximates to that quantity, the index then pointing to an angle nearly = to 180°, the firſt term and the two laſt terms become almoſt evaneſcent, not only from the circumſtances that have been conſidered, but from the quantities $\overline{1-p^2}$, $\sqrt{1-p^2}$, which are multiplied into them. This alſo the obſerver may avail himſelf of: for ſuppoſing he ſhould know, that the angle of incidence on the fixed ſpeculum, and the fixed plane of reflection, are imperfectly adjuſted, and even that the divided arc is incorrect, he may almoſt wholly avoid the errors which they would occaſion, by ſo adjuſting the inclination of the reflectors to the plane of motion, in reſpect of the angle meaſured, that

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for p in the third term, (p. 429.) every thing elſe remaining, the maximum of variation in the obſerved angle on account of 3′ difference in the angle of incidence on the fixed ſpeculum will appear to be = ″.098.

The error of an obſervation occaſioned by a variation $\dot{v}$ in the angle of incidence on the fixed ſpeculum, can never exceed the $\frac{1}{1837}$ part of $\dot{c}$, which is the maximum of error, the angle meaſured being = 22′ 56″ 33‴, the index then pointing to 70° 24′. The error, cauſed by a ſmall angle $\dot{d}$, at which the fixed plane of reflection is inclined to the primitive ſecondary with which it ſhould coincide, cannot exceed the $\frac{1}{14996}$ part of $\dot{d}$, which is its maximum when the angle meaſured is 32′ 33″ 3‴, the index then being directed to 109° 20′.

the index ſhall point to ſome degree near to 180°, which is done by making that inclination very little more than one-fourth of the angle to be obſerved.

33. The tables I. and II. are calculated for taking the diameters of the ſun and planets: the conſtruction being formed on the principles which have been explained (art. 16. 17. 18. 31. 32. fig. 12. 13.). The fixed plane of reflection is coincident with the primitive ſecondary, and conſequently $n=0$: the common inclination of the reflectors to the plane of motion $= 10'$; and the conſtant angle of incidence on the fixed ſpeculum $= 7°$.

TABLE I.

Arc pointed to by the index.	Obſerved angle.			Diff.	
	′	″	‴	″	‴
1°	0	20	47		
				20	47
2°	0	41	34		
				20	47
3°	1	2	21		
				20	47
4°	1	23	8		
				20	46
5°	1	43	54		
				20	46
6°	2	4	40		
				20	45
7°	2	25	25		
				20	45
8°	2	46	10		
				20	44
9°	3	6	54		
				20	43
10°	3	27	37		

TABLE II.

Arc pointed to by the index.	Obſerved angle.			Diff.	
	′	″	‴	″	‴
100°	30	32	51		
				13	28
101°	30	46	19		
				13	20
102°	30	59	39		
				13	12
103°	31	12	51		
				13	3
104°	31	25	54		
				12	54
105°	31	38	48		
				12	46
106°	31	51	34		
				12	37
107°	32	4	11		
				12	28
108°	32	16	39		
				12	19
109°	32	28	58		
				12	11
110°	32	41	9		

The divarication of the images while they traverſe the field of the teleſcope during the time of an obſervation, and the errors of the obſerved angle in conſequence of any change in the quantities m * and n (p. 429) ſhould they appear to be of ſenſible magnitude, may be diminiſhed until they are in phyſical ſenſe evaneſcent, by altering the values of s and m (art. 31.): for this purpoſe it will be requiſite to uſe two ſeparate conſtructions; the one for obſerving very ſmall angles, thoſe, for example, which do not exceed 2′: and the other, for meaſuring ſuch angles as are ſubtended by the ſun and moon. In the former of theſe s may be aſſumed = the ſine of 30″, $m =$ ſin. 7°, and $n = 0$. From theſe data tab. III. is calculated. In the other conſtruction, becauſe very ſmall reflecting ſurfaces are neceſſary to obſerve the ſun by two reflections (art. 17.) m may be aſſumed = ſin. 1°, s being = ſin. 10′, and $n = 0$: from which conditions the fourth table is calculated. Theſe tables may be eaſily extended, by calculating from the value of coſ. ED or $\overline{\text{ſin. } \tfrac{1}{2}\text{ED}}^2$ contained in art. 21. or 22,

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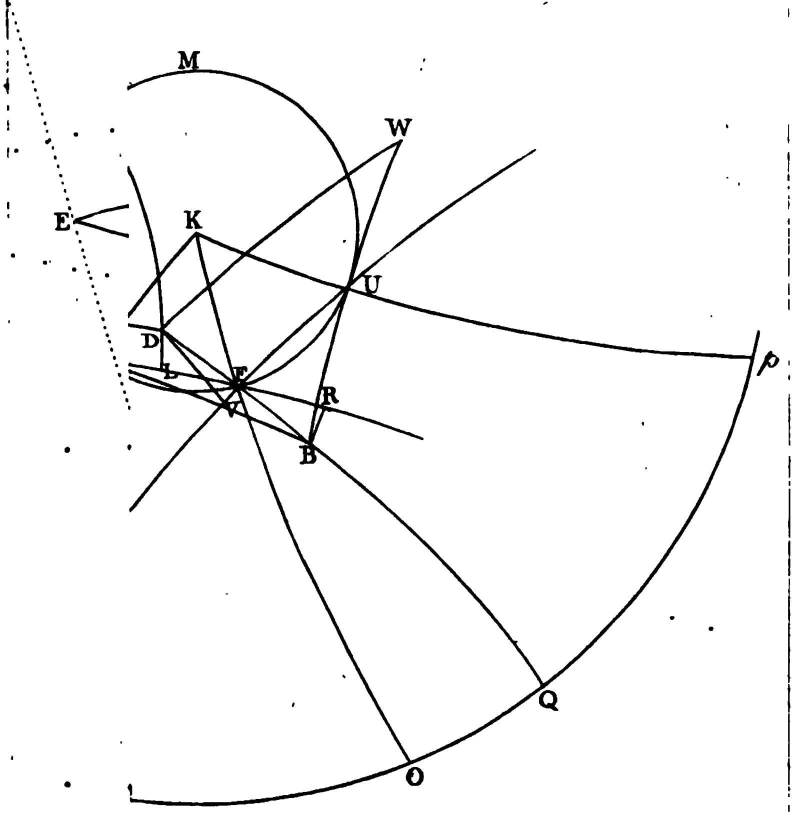
* Let the teleſcope with the entire conſtruction be ſteadily fixed: then if the objects obſerved be in contact while the touching points occupy the center of the field, the angle of incidence on the fixed ſpeculum will be of its true magnitude; that is, its ſine or m will = ſin. 7° in tables I. II. and III.; and $m =$ ſin. 1° in table IV. The fixed plane of reflection alſo will be coincident with the primitive ſecondary in all the conſtructions correſponding to tables I. II. III. and IV. The teleſcope continuing unmoved, the diurnal motion of the heavens will cauſe the points in contact to leave the center of the field; this will occaſion no alteration in the quantities s and p, but will affect m and n only: therefore, $\dot{s}$ and $\dot{p} = 0$. To eſtimate the effects of this change in the values of m and n, let $\dot{m} = \dot{c} \times \sqrt{1 - m^2}$, and $\dot{n} = \dot{d} \times \sqrt{1 - n^2}$, as in art. 28. and ſuppoſe a line to be drawn through the centre of the field in a plane, perpendicular to the plane of motion: this line will be in the fixed plane of reflection; and any deviation of the points in contact through a ſmall angular ſpace $\dot{c}$, from the center of the field will cauſe an equal variation $\dot{c}$ in

the angle of incidence on the fixed ſpeculum; the correſponding ſeparation of the images will be $= \frac{4spm\dot{c} \times \sqrt{\overline{1-m^2} \times \overline{1-p^2}}}{\sqrt{1-m^2+m^2p^2}}$ (p. 429.) which is the greateſt poſſible when $p = \sqrt{\frac{\sqrt{9-10m^2+m^4}-\overline{3-3m^2}}{4m^2}}$ = ſine of 35° 11′ 47″ (if $m =$ ſin. 7°, and $s =$ ſin. 10′, as in tab. I. and II.) being then $= \frac{\dot{c}}{1837}$; but if $s =$ ſin. 30″, as in tab. III. the greateſt ſeparation will be only $\frac{\dot{c}}{36740}$, correſponding to leſs than one-third of a degree for the images motion through 10′ in the field of the teleſcope. In like manner let a line be drawn through the center of the field perpendicular to the line before deſcribed; any deviation of the points in contact through a ſmall angle $\dot{e}$ in the direction of this line will cauſe the plane of reflection at the fixed ſpeculum to be inclined to the primitive ſecondary with which it ſhould coincide at an angle $= \frac{\dot{e}}{m}$; this will occaſion a ſeparation of the images $= \frac{4mp^2s\dot{e} \times \sqrt{1-s^2} \times \sqrt{1-p^2}}{\sqrt{1-m^2+m^2p^2}}$ (page 429. $\frac{\dot{e}}{m}$ being there ſubſtituted for $\dot{d}$) which is the greateſt poſſible when $p = \sqrt{\frac{\sqrt{9-8m^2}-\overline{3-4m^2}}{4m^2}}$. If $m =$ ſin. 7° and $s =$ ſin. 10′, as in tab. I. and II. the greateſt ſeparation of the images will be $= \frac{\dot{e}}{1828} = \frac{\dot{d}}{14996}$: but if $s =$ ſin. 30″ and $m =$ ſin. 7°, as in tab. III. it will only $= \frac{\dot{e}}{36560} = \frac{\dot{d}}{299920}$, which anſwers to a divarication of leſs than 1‴ for the images motion through 10′ in the field of the teleſcope, in the direction of the line above deſcribed, which is drawn through the center of the field, and perpendicular to the fixed plane of reflection. It muſt be remembered, that the ſeparations of the images here eſtimated are greater than can poſſibly happen in theſe conſtructions, when the index is directed to any other points of the circumference of the plane of motion (the diſtance of the images from the center the field not exceeding 10′) and are, even in this caſe, phyſically ſpeaking, evaneſcent.

Fig. III.



Philos. Trans. Vol. LXXI. Tab. XX. p. 434.

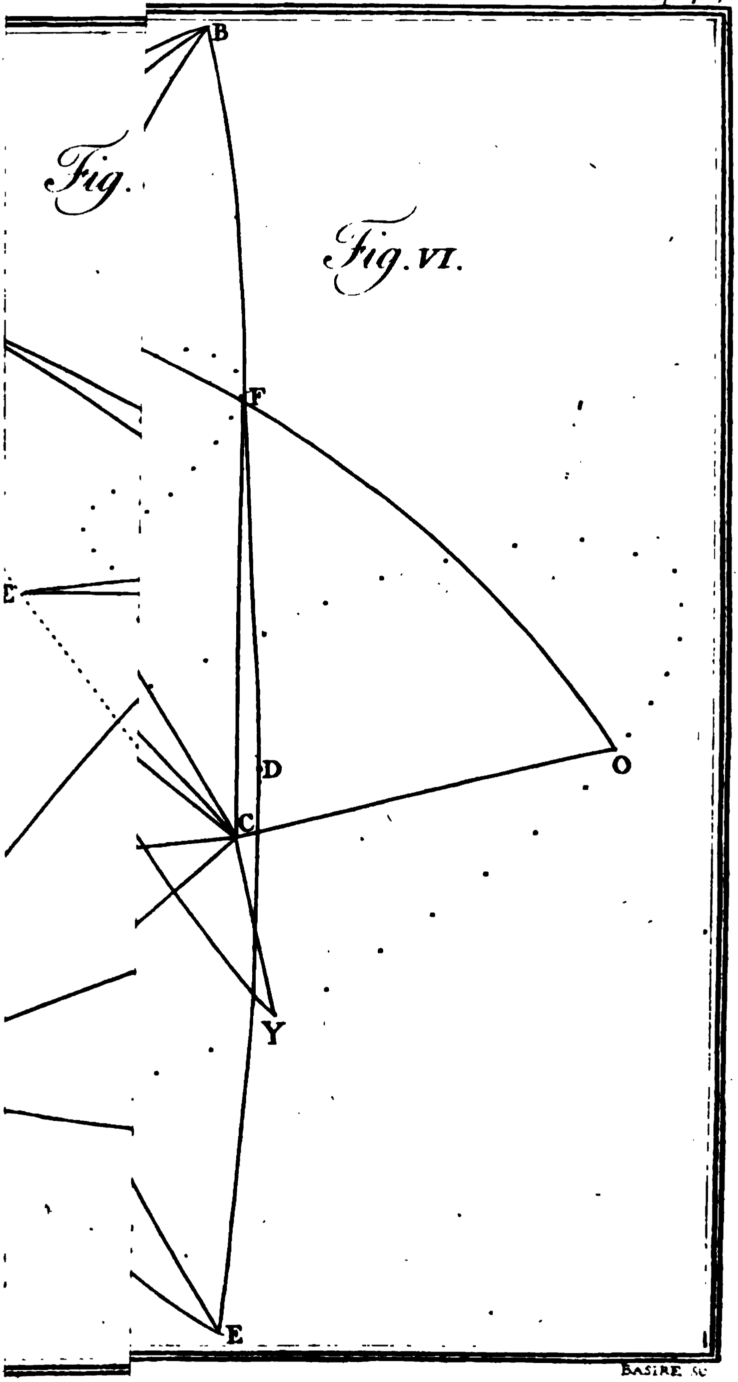
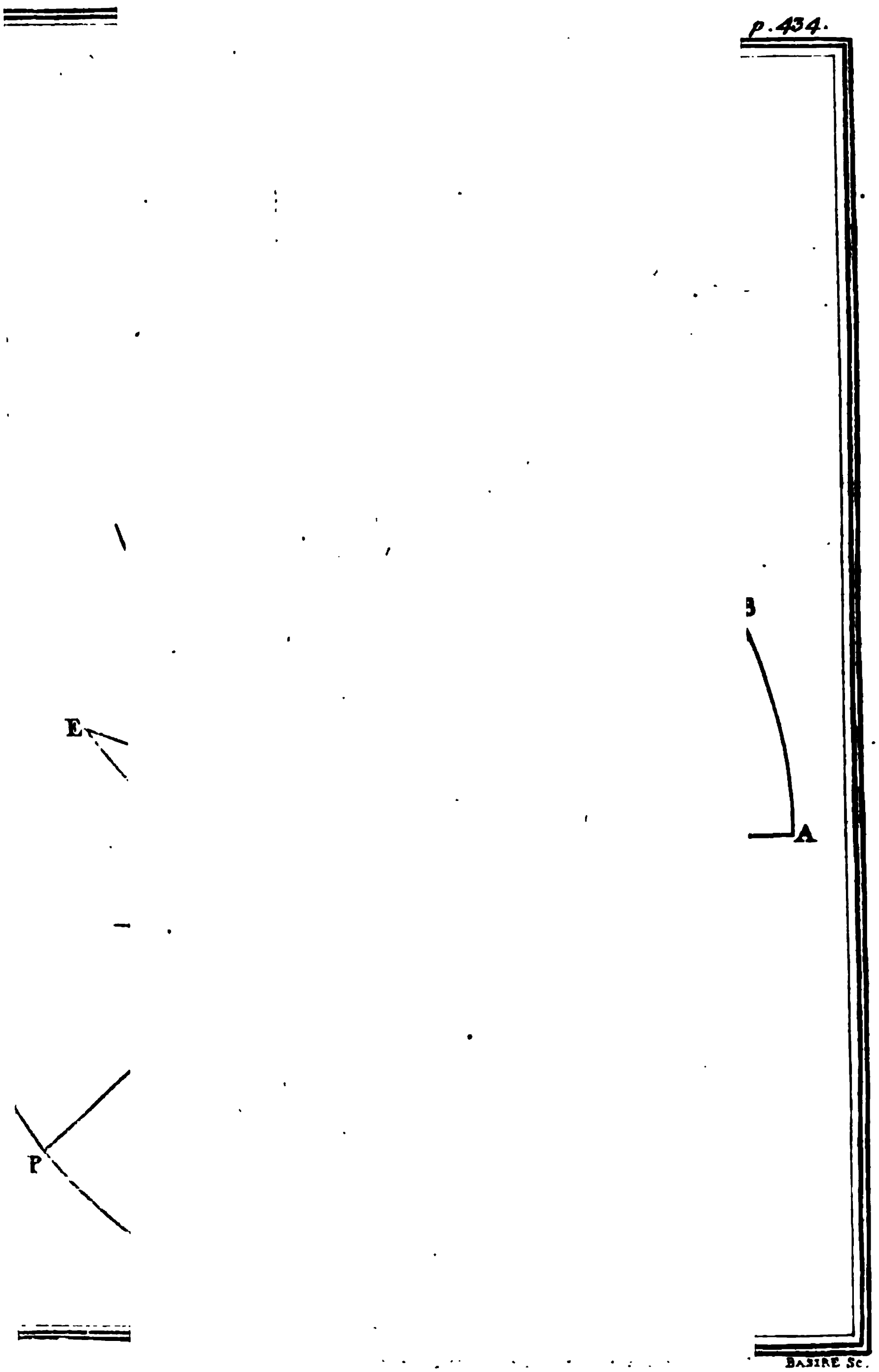


Fig.

Fig. VI.

Basire sc.

p. 434.



Basire Sc.

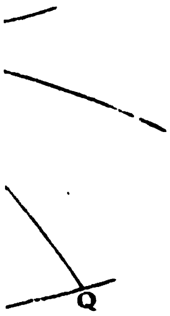


Fig. XV.

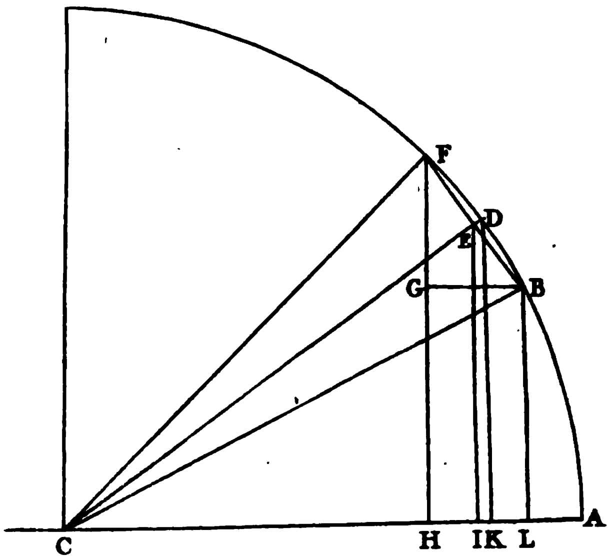


Fig. XVI.

Basire Sc.

TABLE III.

Arc pointed to by the index.	Obſerved angle.			Diff.
	′	″	‴	‴
100°	1	31	39	40
101°	1	32	19	40
102°	1	32	59	40
103°	1	33	39	39
104°	1	34	18	38
105°	1	34	56	38
106°	1	35	34	38
107°	1	36	12	38
108°	1	36	50	37
109°	1	37	27	36
110°	1	38	3	

TABLE IV.

Arc pointed to by the index.	Obſerved angle.			Diff.	
	′	″	‴	″	‴
100°	30	38	23	13	24
101°	30	51	47	13	15
102°	31	5	2	13	7
103°	31	18	9	12	58
104°	31	31	7	12	49
105°	31	43	56	12	41
106°	31	56	37	12	32
107°	32	9	9	12	23
108°	32	21	32	12	14
109°	32	33	46	12	6
110°	32	45	52		

XXVII. *An Account of the* Ophidium barbatum LINNEI. *By* P. M. Auguſtus Brouſſonet, *M. D.*; *communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read July 5, 1781.

THIS ſpecies of fiſh ſeems not to have been unknown to the ancients, though probably they confounded it with the *Conger*, to which it bears ſome reſemblance. Perhaps the early Greek and Latin writers on natural hiſtory have mentioned it under the name of *Tragus*, or *Callarias*; but for want of deſcriptions, they left us much in the dark concerning it. PLINY indeed ſpeaks of a fiſh which appears to be of this ſpecies: he calls it *Ophidion* *, and as that is the name given to it by all the modern writers, we are obliged to accept his ſynonymy without further inquiry.

The firſt author to whom we are indebted for a deſcription and figure of the *Ophidium* is BELLONIUS; yet it appears, that he was not certain of the name of this fiſh, ſince he calls it *Gryllus, falſo congrus, tragus, aſelli ſpecies:* nor was he leſs doubtful of the claſs to which he ſhould refer it, and therefore placed it among the *Aſelli*, or *Gadi*, though very different from the ſpecies of that family. RONDELETIUS, who wrote ſoon after BELLONIUS, has given us a better deſcription, and a more accurate figure of this fiſh, which he calls *Ophidion*, with a reference to PLINY. In the figure of BELLONIUS the cirri are very ill repreſented, and the whole fiſh appears without any ſpots, whereas in the plate of RONDELETIUS it is covered with oblong ſpots. This remarkable difference between the figures of theſe authors was ſufficient to determine GESNERUS, and others who have written ſince their time, and who are to be conſidered rather as compilers than authors, to take the fiſh deſcribed by BELLONIUS to be a different ſpecies from that of RONDELETIUS.

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* Ophidion or Ophidium, though the beſt is Ophidium, is not a good generic name, ſince it is a diminutive from ὄφις.

WILLOUGHBY, who is the firſt ichthyologiſt who has given us any good deſcription of fiſh, treats largely of the *Ophidium*; and in his account deſcribes the ſcales, which are, as we ſhall hereafter explain, oblong, diſtinct, and diſpoſed without any regular order. This deſcription was ſufficient to aſcertain, that the difference between the figures aroſe from RONDELETIUS having drawn the ſcales omitted by BELLONIUS: yet the authors who wrote immediately after WILLOUGHBY, and particularly RAY in his Synopſis, follow GESNERUS, in maintaining two different ſpecies of *cirrata Ophidia*, one with, the other without, ſpots.

ARTEDI did not take notice of the ſpots; he deſcribes the fiſh in a genus to which he gives the name of *Ophidion*, and places that genus among the *Malacopterygii*. After him KLEINIUS once more took notice of the ſpots; but at the ſame time introduced another confuſion concerning this fiſh, ariſing from RONDELETIUS having ſaid, that it has two cirri, while WILLOUGHBY aſſerts it has four; but it is eaſy to reconcile theſe authors, for though the *Ophidium* has only two cirri, yet each of theſe being divided in two, they appear as four; ſo that WILLOUGHBY might juſtly ſay, that it is quadri-cirratus. The ſame author places the *Ophidium* in a genus which he calls

Encbelyopus,

Enchelyopus, which is indeed not a good family, ſince it comprehends the genera of *Gymnotus*, *Anarrhichas*, *Cepola*, *Blennius*, *Cobitis*, &c.

LINNE in his deſcription of the *Ophidium barbatum* ſays, that its whole body is covered with oblong ſpots, without any regular direction. Dr. GOUAN, in his deſcription of the genus of the *Ophidium*, does not mention the ſcales; but gives the ſpots as a generic character. The laſt author who has mentioned theſe ſpots, and given a deſcription of this fiſh, is Mr. BRUNNICHE in his *Ichthyologia Maſſilienſis*.

Having adduced the various opinions of the writers on the *Ophidium*, and endeavoured to reconcile their ſentiments, we now proceed to give the deſcription of this fiſh, which is ſo very remarkable for its ſingularities. The genus of *Ophidium* has the following principal characters, *viz.* the body long; the fins of the back, tail, and anus, confounded in one; no fin on the under part of the body; and the eyes covered by the common ſkin. There are beſides many other characters which it is needleſs to obſerve here, ſince I intend not to deſcribe all the ſpecies of the genus, but only to mention them. The firſt ſpecies (which is the ſpecies of which we are treating) is diſtinguiſhed by its cirri. The ſecond differs from the former not only by the abſence of the cirri, but alſo by many other marks. ARTEDI, in his account of this ſpecies, has adopted the ſynonymy of SCHONEVELDE, who deſcribes a fiſh under the name of *Ophidion imberbe flavum*; but this fiſh, which is the *Blennius gunnellus* LINN. is certainly very different from the *Ophidion imberbe*, LINN. the rays of its dorſal fin being prickly; which circumſtance perhaps induced LINNE to place the *Ophidium* among the *Acanthopterygii* in the firſt editions of his Syſtema, in which he followed the claſſification of ARTEDI. Perhaps, for the ſame reaſon, that author has placed among the *Jugulares* in his *Fauna Suecica*, the ſpecies of which we are now ſpeaking. This fiſh is alſo very different from the *Sea ſnail* deſcribed by WILLOUGHBY and PETIVER, which is the *Cyclopterus lipparis* LINN. though the ſynonymy of PETIVER has been accepted by GRONOVIUS as being the *Ophidium imberbe* *.

LINNE, in the tenth edition of his Syſtema, mentions a third ſpecies of *Ophidium*, which he calls *Macrophthalmum*; but afterwards diſcovered that this ſpecies belonged to the genus of *Cepola*. It is another ſpecies of this genus which OTHO FABRICIUS has lately deſcribed in his *Fauna Grœnlandica*, under the denomination of *Ophidium viride*. There are two other fiſhes, of which GRONOVIUS made a new genus, aſſigning to it the name of *Maſtacembelus*, which comes near to the genus of *Ophidium*, though I am perſuaded it conſtitutes a different one by the preſence of the aculei on the back. I ſhall now proceed to give a deſcription of that ſpecies of which I am particularly treating, which I have written in Latin for the ſake of perſpicuity.

DESCRIPTIO
OPHIDII BARBATI LINN.

B. 7. D. 124. P. 20. V. 0. A. 115. C. — —.

CAPUT compreſſum, ſub-acutum, nudum, cute communi laxe tectum.

Rictus amplus.

Mandibula ſuperior duplicata, inferiore paulo longior.

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* The fiſh mentioned by Mr. PENNANT, in the Britiſh Zoology, tom. III. append. p. 346. and engraved in the tab. 93. tom. II. under the name of *Ophidium imberbe* LINN. is a ſpecies of *Muræna*.

Labia cutacea, tenuia.

Dentes in utriuſque maxillæ margine, in aream anguſtam antice latiuſculam diſpoſiti, minuti, acuti, conferti, anteriores paululum majores.

Lingua ſubobtuſa, glabra.

Palatum medio glabrum, antice areis tribus denticulorum exaſperatum, quarum duæ laterales lineares, intermedia ſubtriangularis.

Fauces ſuperne exaſperatæ duabus areis longitudinalibus denticulorum.

Narium foramina utrinque bina, ante oculos poſita, diſtantia; foramine anteriore minore, poſteriore nudo.

Oculi ſupremi, vicini, magni, cute communi tecti: *iris* argentea; *pupilla* luteſcens.

Opercula branchiarum cute communi tecta, rotundata polyphylla, mollia.

Membrana branchioſtega, cute communi tecta, radiis ſubæqualibus.

Branchiæ quaternæ; tres interiores parte concava gerunt tubercula aſpera, ſeparata, in duas ſeries diſpoſita; quarta cæteris longior, latere interno gerit tubercula aſpera, externo radiis uno latere læviter ſerratos.

Cirri duo ad apicem maxillæ inferioris, bipartiti; *lacinia* altera longiore.

Tuberculum ſubacutum, ſeu apophyſis oſſea tecta, recumbens, ante oculos in fronte poſita.

CORPUS. Compreſſum, verſus caudam attenuatum.

Linea lateralis ſuprema, lævis, dorſo parallela, vitta argentea ſubtus ornata.

Squamæ obovatæ, tectæ, umbonatæ, ſeparatæ.

Pinna dorſalis longa * anali longior ſed anguſtior, cum caudali continuata, baſi ſordide albeſcens, margine nigra e punctis plurimis nigris: *Radii* ſimplices, flexiles, molles.

Pinnæ pectorales obovatæ, pellucidæ, membrana punctis minutiſſimis irrorata.

Pinna analis caudali unita, baſi albeſcens, margine nigra, radiis ſimplicibus.

Pinna caudalis nigra, apice obtuſa.

Color capitis et corporis carneo-argenteus.

Lc. : A :: 85 : 35 Lc. : PI :: 85 : 15
Lc. : DI :: 85 : 27 Lc. : PF :: 85 : 22
Lc. : DFCFAF :: 85 : 87 Lc. : AI :: 85 : 37
R. $5\frac{2}{3}$. P. 2. D. 2. 3. 4. 5. 6. A. 3. 4. 5. 6.

Before I proceed, I think it neceſſary to explain the proportional meaſures which I employed in the foregoing deſcription. ARTEDI conſidered the meaſurement of fiſhes as neceſſary to render the deſcription of them perfect; and therefore in the ſpecies which he deſcribed moſt completely, we find, he did not fail to expreſs, with the greateſt accuracy, the diſtances between the ſeveral parts. He has been followed in this by the beſt authors who have written upon fiſhes, as GRONOVIUS and PALLAS. The like practice has been followed by ſome writers in deſcribing animals of other claſſes; and particularly by Mr. DAUBENTON, who has, with extraordinary exactneſs, expreſſed the meaſurements of quadrupeds. The uſe of this meaſurement, however, can only aſcertain that ſuch or ſuch individual is larger or leſs than another deſcribed by an author: for it is impoſſible by ſuch means to determine a ſpecies, and therefore a ſingle meaſurement, taken from the head to the tail, or in ſhort from any one part to another, would anſwer the ſame purpoſe; but if you take the terms of the meaſures, and compare them one with another, or all of them to a ſingle one, a method may be deduced for determining the ſpecies, and deſcribing each with accuracy, and that is the method which I uſed.

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* Pinna mihi audit longa, cujus baſis longitudine excedit radium altiorem.

There is no doubt that fiſhes have a regular growth, and that all their parts have a proportional increaſe: it is therefore of no conſequence whether the meaſure be taken from an old or a young ſubject, it will be always of the ſame uſe for determining the ſpecies. The term to which I refer all the reſt, is taken from the diſtance between the apex of the upper jaw, and the baſis of the fin of the tail. The extremity of the upper jaw is the point from which I take the diſtance to all the other parts. To determine with more accuracy the junction of the fin to the tail, I incline the fin ſo as to form an angle with the tail; I take the diſtances with a pair of compaſſes, to avoid the irregularities of the ſurface of the body, which are infinitely various. When I have taken the diſtance with the compaſſes, I make a reference to a rule, which is divided in Engliſh inches, each of them ſub-divided in tenths. To exemplify my practice in a fiſh of the ſuppoſed length of 40 lines from the upper jaw to the tail, and from the ſame point to the anus of 20 lines, I ſay, that the diſtance from the head to the tail compared with the diſtance from the head to the anus is in this ſpecies as 40 to 20, which I expreſs in this manner: Lc. : A :: 40 : 20, which I thus reduce, Lc. : A :: 10 : 5. I then take the length from the upper jaw to the beginning of the dorſal * fin, and to its end: next from

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* I meaſure the dorſal and anal fins at their baſis, never in their margin.

the ſame point to the baſis of the pectoral, to its extremity; and in the like manner for the ventral, anal, and tail fins; all which meaſures I refer to the common term 40. Admitting then the dorſal fin is diſtant from the upper jaw as ten, I write as before LC : DI :: 40 : 10, which I reduce, if I chuſe ſo, Lc. : DI :: 20 : 5.

I take the meaſures in length only, and never in breadth or depth, becauſe ſuch meaſures, on many accounts, for inſtance, from the quantity of ſpawn, from that of food in the ventricle, &c. would be uncertain; beſides there are terms enough afforded by the ſituation of the anus and the fins. I pay no regard to the meaſure of the noſtrils, of the eyes, &c. as theſe parts lie too near the extremity of the upper jaw to admit of a ſufficient accuracy in their meaſure. All theſe diſtances are written in the ſhorteſt manner poſſible; I therefore do not go into the inquiry what claſs of the LINNÆAN ſyſtem the fiſh deſcribed may belong to; or whether the anal fin is before or behind the region of the dorſal; whether the pectoral are extended beyond the ventral; or the tail is forked or not; and many other particulars which could not be expreſſed but by a very long deſcription, tedious both to the writer and reader.

To make an application of the proportional meaſurements, I ſuppoſe a fiſh, of a ſpecies deſcribed, to be in length 40 lines, and from the upper jaw to the anus 10: I take a ſpecimen which I ſuſpect to be of the ſame ſpecies, but which I wiſh to determine with certainty: the length of it is 20 lines; then I ſay 40 is to 10 as 20 is to another term, which I diſcover by multiplying 20 by 10, and dividing the product by 40, which will produce 5, the number of the lines comprehended between the upper jaw and the anus, if the fiſh be of the ſpecies it is thought to be. I proceed in the like manner, in comparing the terms of the meaſures of the other parts; but the utility of theſe meaſures is very apparent in the diſtinguiſhing of the ſpecies of ſome genera which are ſo natural as that of *Cyprinus*, *Clupea*, and many others, of which the ſpecies could not be otherwiſe eaſily diſtinguiſhed. I ſhall inſtance a ſpecies of *Perca* deſcribed in the *Commentarii Petropolitani* under the name of *Perca acerina*, by Mr. GUELDENSTAEDT, which could not have been diſtinguiſhed from the *Perca cernua* but by the proportional meaſures; ſince the *Cernua* has the body about three times longer than the head, whereas the *Acerina* has it but twice as long, though by the other characters they are almoſt indiſtinguiſhable.

To expreſs the poſition of the fins briefly, and with all poſſible accuracy, which, however, I think may be very well underſtood by the deſcribed meaſures, I take the diſtance from the upper jaw to the baſis of the pectoral fin, and then ſee into how many equal parts the whole body may be divided, and to theſe parts I apply the name of regions; I meaſure them to the extremity of the middle of the fin of the tail, and I expreſs the poſition of the fins as follow: D. 3. 4. A. 3. V. 2. P. 2. the letters being the initials of the fins, and the numbers of the regions, the firſt being from the head to the pectoral fin *.

The ſcales of the *Ophidium*, which have been figured by RONDELETIUS, but overlooked by many other writers, have been mentioned by WILLOUGHBY, but without any particular deſcription. They are very different from thoſe on the ſkin of the *Ophidium imberbe*, which are ſhortly deſcribed by GRONOVIUS. Their poſition, as may be ſeen in the figure, is irregular. They are diſperſed over the whole body. Their form is ſometimes round, ſometimes nearly oval. They are larger near the head, and in the lower part of the body; but are hardly to be diſtinguiſhed near the tail. They adhere to the body by means of a particular tranſparent ſkin, which is in general very thin, but ſomewhat thicker near the neck, and extended looſely over the whole head: this ſkin is very eaſily deſtroyed, after which the ſcales falling, the body appears ſpotted (fig. 1.). Theſe ſcales are of the ſame ſort as thoſe that LEEUWENHOEK has deſcribed on the eel, like thoſe I have ſeen on the *Anarrhichas lupus*, the *Blennius viviparus*, and many other fiſhes, which are commonly thought to be without ſcales. When you look at them with the naked eye (fig. 2.) they appear as covered with very ſmall grains; but viewed through a microſcope (fig. 3.) the middle of them appears more elevated than the margin; and from the center to the margin, cloſe by each other, there are many lines or rays, formed by ſmall ſcales placed one upon another, like tiles upon a roof, the ſuperior being always the nearer to the center. This ſort of ſcales, which may be called *umbonatæ*, are faſtened to the body by very ſmall veſſels which are inſerted in their middle; they are to be ſeen on the body only, not on the head nor the fins.

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* I beg pardon for the digreſſion; but I thought it would not be improper in this place to obſerve, that the utility of this method of meaſurement will appear not only in diſtinguiſhing fiſhes, but alſo animals of other claſſes, and particularly ſnakes, which cannot be well determined otherwiſe. Beſides, I do not know any author on ſubjects of natural hiſtory, who has adopted that method.

I ſhall now proceed to the anatomy of this fiſh, which certainly comprehends ſome very remarkable circumſtances, which, I believe, have not yet been obſerved in any other ſpecies. When we have drawn off the ſkin there appears a thin membrane of a ſilver colour, which covers the muſcles. The muſcles being removed, we find the peritoneum, which lines the abdominal cavity, and is adherent to the ſwimming bladder by ſome elongations. It is of a ſilver hue, with ſome very ſmall black points. The ventricle is not to be diſtinguiſhed from the inteſtines by any other mark but by its ſize: its form is oblong; it is extended almoſt to the anus, from whence the inteſtinal duct has a retrograde courſe, and then deſcends again, having a little dilatation near the anus. On the vertebræ next the anus on the outſide of the peritoneum is a kind of cavity of an oblong form, containing a reddiſh viſcus, which I take to be the kidney.

The firſt vertebra from the head has nothing very remarkable in its ſtructure. The ſecond has on each ſide an elongated and ſharp apophyſis, to the apex of which is annexed a ſmall ligament. The third is very flat, and has on each ſide a kind of triangular and ſharp apophyſis, to which adheres a ligament as to the ſecond. The fourth is remarkable in having a ſharp apophyſis on each ſide, articulated with the body of the vertebra, and under each of them, is another articulated apophyſis, flattiſh, thick, roundiſh at its extremities, and forked at its baſis (fig. 5.). The fifth, which is ſtrongly adherent to the former, has in its middle a bifid proceſs. The ſixth has in its middle a flattiſh elevation, ſharp on each ſide. Between the extremity of the larger apophyſis of the fourth vertebra, is a bone, or rather a hard cartilage, which bears the figure of a kidney (fig. 6.) its convexity being turned towards the body of the vertebra: its poſition is parallel to the bodies of the vertebræ; its motion is half circular; one of its parts, viz. the loweſt, being in the cavity of the ſwimming bladder, to which it adheres by a thin membrane, ſo that no air can eſcape at that part. It is covered by membranes, which adhere ſtrongly to its middle, in this part are faſtened the two ligaments of the apophyſis of the ſecond and third vertebræ, of which we ſpoke before; and which are of a great tenuity. In the ſame point are faſtened alſo two ligaments each of which belongs to an oblong muſcle parallel to each other, and fixed to the bones of the loweſt and poſterior part of the head (fig. 4.).

All this apparatus is certainly ſubſervient to the purpoſe of ſwimming, I ſuppoſe, by the cavity of the bladder being made larger or leſs by the motions of the cartilagineous bone; but it is very remarkable, that if theſe parts are neceſſary to ſome animal function, they ſhould not be found in all the individuals; for I have ſeen two, of which the vertebræ were not different from the vertebræ of the other ſpecies: which difference depends, perhaps, on the difference of ſex. I am inclined to believe ſo; but the generation in this fiſh ſeems to be no leſs myſterious than that of the eel: I could never diſtinguiſh a male from a female in this ſpecies. I do not know if the other ſpecies of *Ophidium* have the ſame ſtructure; I could not perceive it in ſome ſpecimens of *Maſtacembelus*. WILLOUGHBY mentions that ſingular ſtructure, but without any particular deſcription.

This fiſh commonly grows to the ſize of eight or nine inches. It is to be found in all the Mediterranean Sea, and in great plenty in the Adriatic. It is taken by nets in Provence and Languedoc, together with many other ſmall ſpecies, which are not eſteemed, that is, what they call *Ravaillā*. It is often confounded with the *Cepola* by the fiſhermen, though they have different names for each ſpecies. In Languedoc the *Ophidium* is called *Donzellā*, and the *Cepola*, *flammā*. In Provence the former has the name of *Corrudgiaö*, and the latter that of *Rougeollā*. But the name of *Donzellā*, very common on all the coaſt of the Mediterranean, is alſo applied to the *Cepola*, and the *Sparus julis*

LINN. which, however, is commonly called *girella*. In ſummer the *ophidium* is more common: its fleſh is not of a good taſte, rather coarſe, as that of all the ſpecies of fiſhes, which having no ventral fins, are obliged to make great efforts in ſwimming, and have conſequently the muſcles harder. The want of ventral fins induces me to believe, that it is not a migratory ſpecies. It feeds upon ſmall crabs and fiſhes.

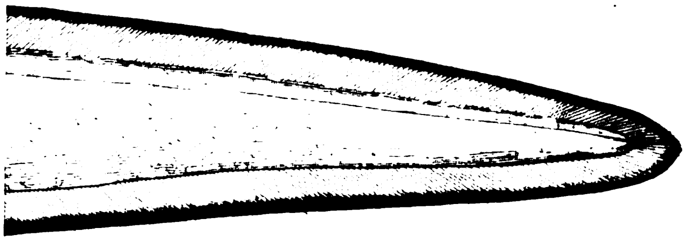

Fig. 5.

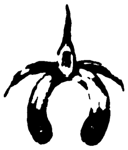

Fig. 6.

Fig. 3.

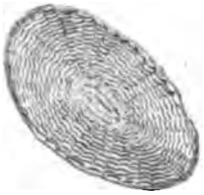

Fig. 2.

barbatum Linn.

XXIX. *A further Account of the Uſefulneſs of waſhing the Stems of Trees. By* Mr. Robert Marſham, *of* Stratton, *F. R. S.*

Read May 31, 1781.

THE following account is a kind of poſtſcript to my letter to Dr. MOSS, Lord Biſhop of Bath and Wells, in 1775, which the Royal Society did me the honour to publiſh in the Philoſophical Tranſactions in 1777. In that I ſhewed how much a Beech increaſed upon its ſtem being cleaned and waſhed[*]; and in this I ſhall ſhew, that the benefit of cleaning the ſtem continues ſeveral years: for the Beech which I waſhed in 1775 has increaſed in the five years ſince the waſhing eight inches and ſix-tenths, or above an inch and ſeven-tenths yearly; and the aggregate of nine unwaſhed Beeches of the ſame age does not amount to one inch and three-tenths yearly to each tree. In 1776 I waſhed another Beech (of the ſame age, *viz.* ſeed in 1741); and the increaſe in four years ſince the waſhing is nine inches and two-tenths, or two inches and three-tenths yearly, when the aggregate of nine unwaſhed Beeches amounted to but one inch and three-tenths and a half. In 1776 I waſhed an Oak which I planted in 1720, which has increaſed in the four years ſince waſhing ſeven inches and two-tenths, and the aggregate of three Oaks planted the ſame year (*viz.* all I meaſured) amounted to but one inch yearly to each tree. In 1779 I waſhed another Beech of the ſame age, and the increaſe in 1780 was three inches, when the aggregate of fifteen unwaſhed Beeches was not full fifteen inches and ſix-tenths, or not one inch and half a tenth to each tree; yet moſt of theſe trees grew on better land than that which was waſhed. But I apprehend the whole of the extraordinary increaſe in the two laſt experiments ſhould not be attributed to waſhing: for in the autumn of 1778 I had greaſy pond-mud ſpread round ſome favourite trees, as far as I ſuppoſed their roots extended, and although ſome trees did not ſhow to have received any benefit from the mud, yet others did, that is, an Oak increaſed half an inch, and a Beech three-tenths, above their ordinary growth. Now though the Beech gained but three-tenths, yet, perhaps, that may not be enough to allow for the mud; for the ſummer of 1779 was the moſt ungenial to the growth of trees of any ſince I have meaſured them, ſome not gaining half their ordinary growth, and the aggregate increaſe of all the unwaſhed and unmudded trees that I meaſured (ninety-three in number of various kinds) was in 1779 but ſix feet five inches and ſeven tenths, or ſeventy-ſeven inches and ſeven-tenths, which gives but eight-tenths and about one-third to each tree; when in 1778 (a very dry ſummer in Norfolk) they increaſed ſeven feet and nine-tenths, or near eighty-five inches, which gives above nine-tenths to each tree: and this ſummer of 1780 being alſo very dry, yet the aggregate increaſe was above half an inch more than in 1778. But the beſt increaſe of theſe three years is low, as there are but twenty of the ninety-three trees that were not planted by me, and greater increaſe is reaſonably expected in young than old trees;

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[*] Vide Phil. Tranſ. vol. LXVII. for the year 1777, part I. p. 12.

yet I have an Oak now two hundred years old* (1780) which is ſixteen feet and five inches in circumference, or one hundred and ninety-ſeven inches in two hundred years. But this Oak cannot properly be called old. The annual increaſe of very old trees is hardly meaſurable with a ſtring, as the ſlighteſt change of the air will affect the ſtring more than a year's growth. The largeſt trees that I have meaſured are ſo far from me, that I have had no opportunity of meaſuring them a ſecond time, except the Oak near the honourable Mr. LEGGE's Lodge in Holt Foreſt, which does not ſhow to be hollow. In 1759 I found it was at ſeven feet (for a large ſwelling rendered it unfair to meaſure at five or ſix feet) a trifle above thirty-four feet in circumference, and in 1778 I found it had not increaſed above half an inch in nineteen years. This more entire remain of longevity merits ſome regard from the lovers of trees, as well as the hollow Oak at Cowthorp in Yorkſhire, which Dr. HUNTER gives an account of in his edition of EVELYN's Silva, and calls it forty-eight feet round at three feet. I did not meaſure it ſo low; but in 1768 I found it at four feet, forty feet and ſix inches; and at five feet, thirty-ſix feet and ſix inches; and at ſix feet, thirty-two feet and one inch. Now, although this Oak is larger near the earth than that in Hampſhire, yet it diminiſhes much more ſuddenly in girt, *viz.* eight feet and five inches in two feet of height (I reckon by my own meaſures as I took pains to be exact). Suppoſe the diminution continues about this rate (for I did not meaſure ſo high) then at ſeven feet it will be about twenty-eight feet in circumference, and the bottom fourteen feet contain ſix hundred and eighty-ſix feet round or buyers meaſure, or ſeventeen ton and ſix feet; and fourteen feet length of the Hampſhire Oak is one thouſand and ſeven feet, or twenty-five ton and ſeven feet, that is, three hundred and twenty-one feet more than the Yorkſhire Oak, though that is ſuppoſed by many people the greateſt Oak in England.

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* I cannot miſtake in the age of this Oak, as I have the deed between my anceſtor ROBERT MARSHAM and the Copyhold Tenants of his Manor of Stratton, dated May 20, 1580, upon his then incloſing ſome of his waſte; and the abuttal is there.

I am unwilling to conclude this account of waſhing the ſtems of trees without obſerving, that all the ingredients of vegetation united, which are received from the roots, ſtem, branches, and leaves of a moſſy and dirty tree, do not produce half the increaſe that another gains whoſe ſtem is clean to the head only, and that not ten feet in height. Is it not clear that this greater ſhare of nouriſhment cannot come from rain? for the dirty ſtem will retain the moiſture longer than when clean, and the nouriſhment drawn from the roots, and imbibed by the branches and leaves, muſt be the ſame to both trees. Then muſt not the great ſhare of vegetative ingredients be conveyed in dew? May not the moſs and dirt abſorb the fineſt parts of the dew? and may they not act as a kind of ſcreen, and deprive the tree of that ſhare of air and ſun which it requires? To develope this myſterious operation of nature would be an honor to the moſt ingenious, and the plain fact may afford pleaſure to the owners of young trees; for if their growth may be increaſed by cleaning their ſtems once in five or ſix years (and perhaps they will not require it ſo often) if the increaſe is but half an inch yearly above the ordinary growth, it will greatly over-pay for the trouble, beſides the pleaſure of ſeeing the tree more flouriſhing. Although the extra increaſe of my firſt waſhed Beech was but four-tenths of an inch, the ſecond was nine-tenths and a half, and the third near two inches, ſo the aggregate extra increaſe is above one inch and one tenth yearly; and the increaſe of the oak is eight-tenths. But calling it only half an inch, then ſix years will produce above five cubic feet of timber, as the oak is eight feet round, and above twenty feet long, and ſix pence will pay for the waſhing, ſo there remains nine ſhillings and ſix pence clear gain in ſix years.

Stratton,
Oct. 29, 1780.

XXX. *Hints relating to the Uſe which may be made of the Tables of natural and logarithmic Sines, Tangents,* &c. *in the numerical Reſolution of affected Equations. By* William Wales, *F. R. S. and Maſter of the Royal Mathematical School in* Chriſt's Hoſpital.

Read June 14, 1781.

THE firſt intimation that I can meet with relating to the uſe which may be made of the tables of ſines, tangents, and ſecants, in reſolving affected equations, is in the latter part of the ſecond volume of Profeſſor SAUNDERSON's Elements of Algebra, printed in 1741, after his deceaſe. The profeſſor there ſhews how to reſolve thoſe two caſes which make the firſt and ſecond of the following examples, by means of the tables; but it appears, from many circumſtances, he was not aware that the third caſe could be reſolved in the ſame manner. All the three forms were, however, reſolved by the late Mr. ANTHONY THACKER, a very ingenious man, who died in the beginning of the year 1744, by the help of a ſet of tables, of his own invention; different from, but in ſome meaſure analogous to, the tables of ſines and tangents. Theſe tables were finiſhed and publiſhed, together with ſeveral papers concerning them, after his death, by a Mr. BROWN of Cleobury. In theſe papers, beſide explaining fully the uſe of the tables in reſolving cubic equations, Mr. THACKER ſhews that his method comprehends the reſolution of all biquadratic equations, if they be firſt reduced to cubic ones in the manner which has been explained by DESCARTES and others; and the ſecond term then taken away.

Since that time M. MAUDUIT has ſhewn how to find the roots of all the three forms of cubic equations, by means of the tables of ſines, &c. in his excellent Treatiſe of Trigonometry. But none of theſe authors have attempted to reſolve equations of more dimenſions than three, by theſe means, without firſt reducing them to that number; nor even theſe, before the ſecond term, or that which involves the ſquare of the unknown quantity, is taken away: whereas ſuch reductions will generally take up more time than is required to bring out the value of the unknown quantity by the following method; and, after all, frequently ſerve no other purpoſe but that of rendering the operation more intricate and troubleſome.

The truly ingenious Mr. LANDEN, in his lucubrations, publiſhed in 1755, has given a general method of reſolving that caſe of cubic equations, by means of the tables of ſines, where all the roots are real, without the trouble of taking away the ſecond term of the equation: and Mr. SIMPSON has ſhewn how to reſolve equations of any dimenſions, by the ſame means, provided thoſe equations involve only the odd powers of the unknown quantity, and that the co-efficients obſerve ſuch a law as will reſtrain the equation to that form which is expreſſive of the coſine of the multiple of an arc, of which the unknown quantity is the coſine. This was firſt done, I believe, by JOHN BERNOULLI, and afterwards by Mr. EULER, in his *Introduct. ad Analyt. Infinit.* and Mr. DE MOIVRE, in his *Miſcell. Analyt.*; but the reſolution of all equations of this form, as well as many others, is comprehended in the firſt of the following obſervations.

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The firſt thought of extending the uſe of the tables of ſines, tangents, and ſecants, farther than to the caſes which have been already mentioned, occurred to me while I was conſidering the problem which produced the equation given in this paper as the fourth example. And it is remarkable, that the very ſame thought occurred to Dr. HUTTON about the ſame time, and in the reſolution of the ſame problem; and we were not a little ſurprized, on comparing our ſolutions together, to find that our ideas had taken ſo exactly the ſame turn; and that both ſhould have ſtumbled on a thought, which, as far as either of us knew, had never preſented itſelf to any one before. Having ſince examined farther into the matter, I have the ſatisfaction to find, that the principle is very extenſive, and that a great number of equations, eſpecially ſuch as ariſe in the practice of geometry, aſtronomy, and optics, may be reſolved by it with great eaſe and expedition.

But beſide the facility with which the value of the unknown quantity is brought out by means of the tables of ſines, tangents, and ſecants, this method of reſolution has another conſiderable advantage over moſt others which have been propoſed, inaſmuch as the firſt ſtate of the equation, without any previous reduction, is generally the beſt it can be in for reſolution; and from which it may moſt readily be diſcovered, how to ſeparate it into ſuch parts as expreſs the ſine, or the tangent, or the ſecant of the arc of a circle; or into the ſine, tangent, or ſecant of ſome multiple of that arc, or of a part of it; and in the doing of which conſiſts the principal part of the buſineſs in queſtion. It will alſo be of ſome advantage to preſerve the original ſubſtitutions as diſtinct as poſſible, by uſing only the ſigns of the ſeveral operations which it may be neceſſary to go through in bringing the ſolution of a problem to an equation, inſtead of performing the operations themſelves.

Beſides the advantages which this method of procedure affords to the mode of reſolution now more particularly under conſideration, it has ſo many others over that which is commonly made uſe of, that I am much ſurprized the latter ſhould ever have been adopted. By preſerving thus the original ſubſtitutions diſtinct, all the way through an operation, every expreſſion, even to the final equation, will exhibit the whole proceſs up to that ſtep; and it will appear as clearly, how every expreſſion has been derived, as it does in that mode of analyſis which was uſed by the ancient geometricians: whereas, when the ſeveral original expreſſions are melted down into one maſs by the multitude of actual additions, ſubtractions, multiplications, and diviſions, which they generally undergo, in a long algebraical proceſs, conducted in the uſual manner, it is impoſſible to trace the ſmalleſt veſtige of the original quantities in the final equation, except ſuch as are repreſented by a ſingle letter. Of courſe, however obvious the ſeveral ſteps might be at the time when they were taken, every idea of them muſt be totally loſt in the reſult; and it will be utterly impoſſible to trace them back again, in the manner they are done in the compoſition of a problem, the ſolution of which has been inveſtigated by the geometrical analyſis *. Let me add, that it is to this cauſe

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* This ſubject, if ever I am bleſſed with more leiſure than is at preſent my lot, ſhall be purſued farther in another paper: in which I ſhall endeavour to ſhew, that, notwithſtanding the great difference which there appears to be between algebra and geometry, they are really but one ſcience, differently treated; and that the operations of the former may be rendered as clear and perſpicuous as thoſe of the latter are allowed to be. A diſquiſition of this nature will at leaſt have the merit of reſcuing a very uſeful and expeditious mode of inveſtigation from

cauſe we muſt attribute all that obſcurity which the algebraic mode of inveſtigation has been ſo frequently charged with.

I ſhall endeavour to verify this doctrine, in ſome meaſure, by the expreſſions which are put down in the following tables for the ſines, coſines, and tangents of arcs of circles, and of the multiples of thoſe arcs; which tables will be found very uſeful in the proſecution of the deſign which I am now upon, and are abſolutely neceſſary in the explanation of it.

from an unmerited ſtigma: and if I never be happy enough to have an opportunity of doing it myſelf, what I have here ſaid may be the means of putting ſome other perſon, who has, upon it.

A	Tangent.
	$\frac{rx}{\sqrt{\overline{r+x}\cdot\overline{r-x}}}$
	$\frac{2}{\overline{r+x}\cdot\overline{r-x}-x^2}\,\frac{rx}{\sqrt{\overline{r+x}\cdot\overline{r-x}}}$
	$\frac{3\cdot\overline{r+x}\cdot\overline{r-x}-x^2}{\overline{r+x}\cdot\overline{r-x}-3x^2}\times\frac{rx}{\sqrt{\overline{r+x}\cdot\overline{r-x}}}$
	$\frac{4\cdot\overline{\overline{r+x}\cdot\overline{r-x}-4x}^2\quad rx}{\overline{r+x}\rvert^2\cdot\overline{r-x}\rvert^2-6x^2\cdot\overline{r+x}\cdot\overline{r-x}+x^4\sqrt{\overline{r+x}\cdot\overline{r-x}}}$
	$\frac{5\cdot\overline{r+x}\rvert^2\cdot\overline{r-x}\rvert^2-10x^2\cdot\overline{r+x}\cdot\overline{r-x}+x^4}{\overline{r+x}\rvert^2\cdot\overline{r-x}\rvert^2-10x^2\cdot\overline{r+x}\cdot\overline{r-x}+5x^4}\times\frac{rx}{\sqrt{\overline{r+x}\cdot\overline{r-x}}}$
	$\frac{\cdot\overline{r+x}\rvert^2\cdot\overline{r-x}\rvert^2-20x^2\cdot\overline{r+x}\cdot\overline{r-x}+6x^4\quad rx}{\cdot\overline{r-x}\rvert^3-15x^2\cdot\overline{r+x}\rvert^2\cdot\overline{r-x}\rvert^2+15x^4\cdot\overline{r+x}\cdot\overline{r-x}\cdot\sqrt{\overline{r+x}\cdot\overline{r-x}}}$

A.	Tangent.
1	x
2	$\frac{2r^2x}{\overline{r+x}\cdot\overline{r-x}}$
3	$\frac{x\cdot\overline{r+x}\cdot\overline{r-x}+2r^2x}{\overline{r+x}\cdot\overline{r-x}-2x^2}$
4	$\frac{4r^2x\cdot\overline{r+x}\cdot\overline{r-x}}{\overline{r+x}\rvert^2\cdot\overline{r-x}\rvert^2-4r^2x^2}$
5	$\frac{x\cdot\overline{r+x}\rvert^2\cdot\overline{r-x}\rvert^2+4r^2x\cdot\overline{r+x}\cdot\overline{r-x}-4r^2x^3}{\overline{r+x}\rvert^2\cdot\overline{r-x}\rvert^2-4x^2\cdot\overline{r+x}\cdot\overline{r-x}-4r^2x^2}$
6	$r^2\cdot\frac{6x\cdot\overline{r+x}\rvert^2\cdot\overline{r-x}\rvert^2-8r^2x^3}{\overline{r+x}\rvert^3\cdot\overline{r-x}\rvert^3-12r^2x^2\cdot\overline{r+x}\cdot\overline{r-x}}$

Tangent.
$\sqrt{\overline{x+r}\cdot\overline{x-r}}$
$\dfrac{2r^2\cdot\sqrt{\overline{x+r}\cdot\overline{x-r}}}{r^2-\overline{x+r}\cdot\overline{x-r}}$
$\dfrac{3r^2-\overline{x+r}\cdot\overline{x-r}}{r-3\cdot\overline{x+r}\cdot\overline{x-r}}\sqrt{\overline{x+r}\cdot\overline{x-r}}$
$\dfrac{4r^4-4r^2\cdot\overline{x+r}\cdot\overline{x-r}}{r^4-6r^2\cdot\overline{x+r}\cdot\overline{x-r}+\overline{x+r}\vert^2\cdot\overline{x-r}\vert^2}\sqrt{\overline{x+r}\cdot\overline{x-r}}$
$\dfrac{5r^4-10r^2\cdot\overline{x+r}\cdot\overline{x-r}+\overline{x+r}\vert^2\cdot\overline{x-r}\vert^2}{r^4-10r^2\cdot\overline{x+r}\cdot\overline{x-r}+5\cdot\overline{x+r}\vert^2\cdot\overline{x-r}\vert}\sqrt{\overline{x+r}\cdot\overline{x-r}}$
$\dfrac{6r^6-20r^4\cdot\overline{x+r}\cdot\overline{x-r}+6r^2\cdot\overline{x+r}\vert^2\cdot\overline{x-r}\vert^2}{{}^6-15r^4\cdot\overline{x+r}\cdot\overline{x-r}+15r^2\cdot\overline{x+r}\vert^2\cdot\overline{x-r}\vert^2-\overline{x+r}\vert^3\cdot\overline{x-r}\vert^3}\sqrt{\overline{x+r}\cdot\overline{x-r}}$

Tangent.
$\dfrac{r}{r-x}\sqrt{r^2-\overline{r-x}\vert^2}$
$\dfrac{2r\cdot\overline{r-x}}{2\cdot\overline{r-x}\vert^2-r^2}\sqrt{r^2-\overline{r-x}\vert^2}$
$\dfrac{4r\cdot\overline{r-x}\vert^2-r^3}{4\cdot\overline{r-x}\vert^3-3r^2\cdot\overline{r-x}}\sqrt{r^2-\overline{r-x}\vert^2}$
$\dfrac{8r\cdot\overline{r-x}\vert^3-4r^3\cdot\overline{r-x}}{8\cdot\overline{r-x}\vert^4-8r^2\cdot\overline{r-x}\vert^2+r^4}\sqrt{r^2-\overline{r-x}\vert^2}$
$\dfrac{16r\cdot\overline{r-x}\vert^4-12r^3\cdot\overline{r-x}^2+r^5}{16\cdot\overline{r-x}\vert^5-20r^2\cdot\overline{r-x}\vert^3+5r^4\cdot\overline{r-x}}\sqrt{r^2-\overline{r-x}\vert^2}$
$\dfrac{32r\cdot\overline{r-x}\vert^5-32r^3\cdot\overline{r-x}\vert^3+6r^5\cdot\overline{r-x}}{32\cdot\overline{r+x}\vert^6-48r^2\cdot\overline{r-x}\vert^4+18r^4\cdot\overline{r+x}^2-r^6}\sqrt{r^2-\overline{r-x}\vert^2}$

Obſervations on the foregoing tables.

EACH of the formulæ in theſe tables may be conſidered as one ſide of an equation, involving the unknown quantity x to different dimenſions. In ſome of the formulæ the odd powers of x are only found, in others the even ones alone, and in others both; but they are all equally uſeful in finding the value of the unknown quantity in adfected equations which contain all the powers of that quantity, as will plainly appear from the following conſiderations.

I. If, on bringing the ſolution of any problem to an equation with ſome known quantity, it be found to correſpond with any of the formula in theſe tables; or, if by any means it can be reduced to any of them, it is manifeſt, that nothing remains to be done but to divide the known ſide of the equation by the value of the quantity which is here denoted by r, and to ſeek for the quotient in the tables of ſines, coſines, or tangents, as the caſe may require, and the value of the unknown quantity will be the ſine, tangent, ſecant, or verſed ſine, of a given part of that arc (according as the expreſſion is found in the firſt, ſecond, third, or fourth table) multiplied by the value of r.

II. If, as will more frequently happen, the final equation of an operation be found equivalent to the ſum, difference, product, or quotient, of ſome two or more of theſe formula; or to the ſum, difference, product, or quotient, of ſome two or more of them multiplied or divided, increaſed or leſſened, by ſome known quantity or quantities; then, having taken away the known quantities by the common algebraic rules, obſerve the following ones.

1ſt. When the equation is found to correſpond with the ſum or difference of two formula in theſe tables, which are the ſine and tangent, ſine and coſine, or coſine and tangent, of the ſame arc, by running the eye along the tables of natural ſines and tangents, find theſe two arcs, immediately following one another, the ſum or difference of the ſine and tangent, ſine and coſine, or coſine and tangent, of which are one of them greater, and the other leſs than the number which conſtitutes the known ſide of the equation. Take the exceſs of one of theſe ſums or differences above, and what the other ſum or difference wants of the ſaid given number, add theſe two errors together, and ſay, as the ſum of them is to 60″, ſo is that error which belongs to the leſs arc to a number of ſeconds; which being added to the leſs arc will give one, the ſum or difference of whoſe ſine and tangent, ſine and coſine, or coſine and tangent, is exactly equal to the number which conſtitutes the known ſide of the equation. Of the arc, thus found, let ſuch a part be taken as the table in which the formula are found directs, and the natural ſine, tangent, ſecant, or verſed ſine (as the caſe may require) of this part, being multiplied by the value of r, if r be found in the equation, will be the value of x ſought.

2d. When the equation happens to be the product or quotient of two formulæ which expreſs the ſine and coſine, ſine and tangent, or coſine and tangent, of the ſame arc, take the logarithm of the number which conſtitutes the known ſide of the equation, and then follow exactly the directions given in the firſt caſe, uſing the tables of logarithmic ſines and tangents inſtead of the tables of natural ones.

3d. If the equation, finally reſulting from the reſolution of any problem, preſent itſelf in an expreſſion which is compoſed of the ſum or difference of the ſine, coſine, or tangent, of an arc, of which the unknown quantity is the ſine, coſine, tangent, or verſed ſine, and the ſine, coſine, or tangent, of ſome multiple of that arc, it will then be convenient to have two tables of ſines and tangents; and in running the eye along them to find the two arcs immediately following one another, of which the ſum or difference of the ſine, coſine, or tangent, of one of them, and the ſine, coſine, or tangent, of ſome multiple of it, may be leſs, and the ſum or difference of the ſine, coſine, or tangent, of the other, and the ſine, coſine, or tangent, of the ſame multiple of it, may be greater than the number which conſtitutes the known ſide of the equation, for every minute of a degree that the finger is moved over in one, it muſt be moved over a number of minutes in the other, which is equal to the number of times that the ſingle arc is contained in the multiple one. When theſe two arcs are found, the operation will not differ ſo materially from that which is pointed out in the firſt rule as to merit repetition.

4th. If, inſtead of the ſum or difference of the ſine, coſine, or tangent, of an arc, and the ſine, coſine, or tangent, of ſome multiple of it, the form of the equation be ſuch as to be conſtituted of the product of them, or the quotient of one divided by the other, the laſt rule will ſtill hold good, uſing only the logarithmic ſines and tangents inſtead of the natural ones, and comparing the ſum or difference of them, according as the equation is compoſed of the product or quotient of the two factors, with the logarithm of the number which conſtitutes the known ſide of the equation, inſtead of that number itſelf.

5th. Sometimes the final equation will come out in expreſſions which are conſtituted of the ſum, difference, product, or quotient, of the ſine, coſine, or tangent, of ſome multiple of an arc, of which the unknown quantity is the ſine, tangent, ſecant, or verſed ſine, and the ſine, coſine, or tangent, of ſome other multiple of the ſame arc. And in any of theſe caſes it is manifeſt, that the method of proceeding, in order to obtain one of the multiple arcs, and from thence the ſingle one, of which the unknown quantity is the ſine, tangent, &c. will not be greatly different from thoſe which have been deſcribed in the third and fourth rules. The moſt material difference conſiſts in this, that inſtead of proceeding minute by minute, according to the directions in the third rule to find the ſingle arc, it will now be moſt convenient to proceed in each table by as many minutes at each ſtep as are equal in number to the number of times which the ſingle arc is contained in the multiple ones reſpectively.

6th. Equations will frequently make their appearance in formulæ which expreſs the ſquare, cube, &c. of the ſine, coſine, or tangent, of the multiple of ſome arc, of which the unknown quantity is the ſine, tangent, ſecant, or verſed ſine; or in formulæ which are expreſſive of the ſum, difference, product, &c. of the ſine, coſine, or tangent, of an arc, and ſome power of the ſine, coſine, or tangent, of the ſame arc; or of ſome multiple of it, the unknown quantity being ſome other trigonometrical line belonging to that arc. Or the equation may be compounded of the ſum, difference, product, &c. of the ſame, or different powers of the ſines, tangents, or coſines, of different multiples of an arc, the unknown quantity being the ſine, tangent, ſecant, or verſed ſine, of that arc. In every one of theſe caſes the tables will give the value of the unknown quantity, and in moſt of them with great eaſe and expedition. The method which is to be purſued in each caſe will readily preſent itſelf to a ſkilful analyſt, who attends carefully to what has been already ſaid, and to the examples which follow.

IV. The formula in the four preceding tables may be greatly varied by ſuppoſing x, the unknown quantity, to be ſome part or parts of the ſine, tangent, &c. as $\frac{1}{2}$, $\frac{1}{3}$, $\frac{2}{3}$, $\frac{3}{2}$, &c. or ſome multiple of it, as twice, thrice, &c. Or x may be the ſquare, or the ſquare root, or any other power of the ſine, tangent, ſecant, or verſed ſine, of an arc; in every one of which caſes the formula will put on different appearances, either with reſpect to the powers or co-efficients of the unknown quantity, and yet admit of the ſame kind of application.

V. The tables may be rendered yet more extenſively uſeful by inſerting expreſſions for the ſines, coſines, and tangents, of half the arc which has x for its ſine, tangent, ſecant, or verſed ſine; and alſo for the ſines, coſines, and tangents, of the odd multiples of this half arc, which expreſſions, together with thoſe already inſerted, may be conſidered as the ſines, coſines, and tangents, of the multiples of an arc, the unknown quantity, being the ſine, tangent, &c. of twice that arc. And this conſideration may ſometimes be applied to very uſeful purpoſes.

VI. In order to render the formula in the tables more general, I have put r for the radius of the circle; whereas it will frequently happen, that the equation, finally reſulting from the reſolution of a problem, eſpecially thoſe which relate to the doctrine of the ſphere, will preſent itſelf in a form where the radius muſt be taken equal to unity: what theſe forms are will readily appear by ſubſtituting unity for r and its powers every where in the expreſſion.

It would be endleſs were I to undertake to enumerate all the various circumſtances and caſes in which this method of bringing out the unknown quantity may be applied with ſucceſs: what has already been ſaid will be ſufficient to explain the nature of it, and to enable the analyſt to apply it in other inſtances as they occur to him, I ſhall therefore only add a few examples to illuſtrate it.

EXAMPLE I.

Let it be required to find the value of x in an equation of the form $x^3 - r^2x = a$.

If r^2 be expounded by 50, and a by 120 (ſee Phil. Tranſ. vol. LXVIII. p. 937.) the equation may be reduced to $\sqrt{x} \times \sqrt{x^2 - 50} = \sqrt{120}$; and, conſequently, by tab. III. if x be conſidered as the ſecant of an arc, of which the radius is $\sqrt{50}$, $\sqrt{x^2 - 50}$ will be the tangent of it, and we ſhall have to find an arc, ſuch that the tangent multiplied by the ſquare root of the ſecant may be equal to $\sqrt{120}$; or, which amounts to the ſame thing, ſuch an arc that the log. tang. together with half the log. ſecant may be equal to half the log. of 120. But becauſe the tangent and ſecant, here required, are to the radius of the $\sqrt{50}$, the log. tangents and ſecants in the tables muſt be increaſed by the logarithm of that number, and therefore log. tang. + ½ log. 50 + ½ log. ſecant + ¼ log. 50 = ½ log. 120: or log. tang. + ½ log. ſecant = ½ log. 120 − ¾ log. of 50. Hence, having taken ¾ the log. of 50 from ½ the log. of 120, run the eye along the tables of logarithmic tangents and ſecants until an arc be found of which the ſum of the log. tangent and half the log. ſecant is equal to 19.7653631, the remainder.

In this manner it will be readily found, that the ſum of the log. tangent and half the log. ſecant of 28° 37′ is leſs than that difference by 2012, and that the ſum of the log. tangent and half the log. ſecant of 28° 38′ is greater than it by 1337: therefore 3349 (2012 + 1337) : 60″ :: 2012 : 36″. The exact arc, therefore, of which the ſum of the log. tangent and half the log. ſecant is equal to 19.7653631 is 28° 37′ 36″, and the log. ſecant of it is 10.0566242, which being increaſed by 0.8494850, the log. of $\sqrt{50}$ gives 0.9061092, which is the logarithm of 8.055810, the value of x ſought, and which is true to ſeven places of figures.

EXAMPLE II.

To find the value of x in an equation of the form $x^3 - r^2x = -a$.

If r be expounded by 3, and a by 10, as they are in the example, given at p. 433. of the Phil. Tranſ. vol. LXX. the equation will be $x^3 - 9x = -10$, and may be transformed to $\sqrt{x} \times \sqrt{9 - x^2} = \sqrt{10}$; and, therefore, by tab. I. the ſquare root of the ſine into the coſine of an arc, of which the radius is 3, is equal to the ſquare root of 10. Conſequently, an arc muſt be found, ſuch that the ſum of the log. coſine and half the log. tangent is equal to half the log. of 10. But becauſe the radius of this arc muſt be 3, the log. ſines and coſines muſt be increaſed by the log. of 3; and, therefore, log. coſ. + log. of 3 + ½ log. ſine + ½ log. of 3 muſt be equal to half the log. of 10; or, an arc muſt be found of which the ſum of the tabular log. coſine and half the log. ſine is equal to the difference between half the log. of 10 and 1½ the log. of 3. Hence, having ſubtracted 1½ log. of 3 from half the log. of 10, run the eye along GARDINER's tables of logarithmic ſines, by which means it will be readily found, that the ſum of the log. coſine and half the log. ſine of 28° 53′ 30″ is leſs than 19.7843181, the exceſs of half the log. of 10 above 1½ log. 3, by 15, and that the ſum of the log. coſine and half the log. ſine of 28° 53′ 40″ is greater than that difference by 60. Conſequently 75 (15 + 60) : 10″ :: 15 : 2″. The exact arc, therefore, of which the ſum of the log. coſine and half the log. ſine is equal to 19.7843181, is 28° 53′ 32″; and the log. ſine of this arc, increaſed by the log. of 3, is 0.1612153, the logarithm of 1.44949, the value of x required, true to the laſt place.

But many equations of this form, and this example among the reſt, admit of two poſitive values of the unknown quantity; and by carrying the eye farther along the tables it will be found alſo, that the ſum of the log. coſine and half the log. ſine of 41° 48′ 30″ is greater than 19.7843181 by 50, and that the ſum of the log. coſine and half the log. ſine of 41° 48′ 40″ is too little by 21. Conſequently, 71 (50 + 21) : 10″ :: 50 : 7″: of courſe, 41° 48′ 37″ is another arc, of which the ſum of the log. coſine and half the log. ſine is equal to 19.7843181, and the log. ſine of this arc, increaſed by the log. of 3, is the logarithm of 1.999999, another value of x, and which errs but by unity in the ſeventh place.

The third root, as it is generally called, of this equation, which is neceſſarily negative, and equal to the ſum of the other two, belongs properly to the equation which is given as the firſt example, of which it is the affirmative root, and may be found by the directions which are there given.

EXAMPLE III.

To find the value of x in an equation of the form $x^3+r^2x=a$.

Let us take as examples of this equation $x^3+3x=.04$, $x^3+3x=.08$, and $x^3+3x=.12$, which are three of the inſtances given by Dr. HALLEY, in his Synopſis of the Aſtronomy of Comets, to illuſtrate the mode of computation that he purſued in conſtructing his general table for calculating the place of a comet in a parabolic orbit: and it is obvious, a being put for the known ſide of the equation, that it may be transformed to $\sqrt{x}\times\sqrt{3+x^2}=\sqrt{a}$: where, if x be conſidered as the tangent of an arc, the radius of which is $\sqrt{3}$, $\sqrt{3+x^2}$ will be the ſecant of that arc; and, conſequently, by what is ſhewn in the firſt example, an arc muſt be found, ſuch, that the ſum of the tabular log. ſecant and half the tabular log. tangent may be equal to the exceſs of half the log. of a above ¼ of the log. of 3. In the firſt of the above three inſtances this exceſs will be found, 18.9431891, in the ſecond 19.0937041, and in the third 19.1817497; and by running the eye along GARDINER's Tables of Logarithmic Sines and Tangents, it will be found, that the firſt falls between 0° 26′ 20″ and 0° 26′ 30″, the ſecond between 0° 52′ 50″ and 0° 53′ 0″, and the third between 1° 19′ 20″ and 1° 19′ 30″; and, by purſuing the mode which has been deſcribed in the two former examples, the exact arcs will be found 0° 26′ 27″,7, 0° 54′ 51″,7, and 1° 19′ 20″,1, and their reſpective tangents, to the radius $\sqrt{3}$, .01333248, .0266611, and .0399787, the three values of x ſought. And in this manner Dr. HALLEY's table may be extended to any length with the utmoſt eaſe, expedition, and accuracy.

3 Thus

Thus far this matter has been carried by former writers; but thoſe who may be at the trouble of conſulting them will find that I have not copied their methods: on the contrary, theſe which are given here are more plain and obvious than theirs are, and the operations conſiderably ſhorter. What follows has not, I believe, been adverted to by any before me.

EXAMPLE IV.

Let the equation ariſing from the proportion $a : b + x \,.\, \overline{1 - c^2} :: c\sqrt{1 - x^2} : c^2 x$ be taken, which is the reſult of an inquiry into the ſituation of that place on the ſurface of the earth, conſidered as a ſpheroid, which is at the greateſt diſtance from a given one, ſuppoſe London. In this inquiry a and b were put to repreſent the ſine and coſine of the latitude of the given place (in the ſpheroid); c for $\frac{229}{230}$, the ratio of the axes; and x for the ſine of the diſtance of the required place from the oppoſite pole (in the ſpheroid alſo). The equation, which is of four dimenſions with all the terms, is manifeſtly $acx = \overline{b + x \,.\, 1 - c^2} \times \sqrt{1 - x^2}$, or $\frac{x}{\sqrt{1 - x^2}} - x \,.\, \frac{1 - c^2}{ac} = \frac{b}{ac}$; in which it is evident from tab. I. that the difference between the tangent and the product of the ſine into a given quantity is known. In order, therefore, to find the value of x, compute $\frac{b}{ac}$, and $\frac{1 - c^2}{ac}$, and find the logarithm of the latter. Now, becauſe the elliptic meridian differs but little from a circle, the place ſought will not be far from the antipodes of the given one, and its diſtance from the oppoſite pole may therefore be eſtimated at

39° 5′;

39° 5′; and, having taken out the natural tangent, and logarithmic ſine of this arc, add the logarithm of $\frac{1-c^2}{ac}$ to the latter, and find the number correſponding to the ſum, which will be leſs than the natural tangent of 39° 5′ by 2869. As this aſſumption is ſo near, take 39′ 6″ for the next, repeat the operation, and the reſult will be 1935 too great. Then 4804 $(2869+1935) : 60'' :: 2869 : 36''$; which being added to 39°5′, gives 39° 5′ 36″, for the co-latitude of the place ſought, and the natural ſine of this arc, or .6305856 is the value of x in this equation.

EXAMPLE V.

Let the equation $x^3 + \frac{b^2-2a^2}{4a}x^2 + \frac{2a^2-b^2}{2}x - \frac{ab^2}{4} = 0$, be taken, which reſults from a ſolution of one caſe of the problem *de inclinationibus* of APOLLONIUS; but which, as it naturally riſes to a ſolid problem, was not, I conceive, conſidered by that celebrated author. The reſult of the analyſis, before any reduction takes place, is this proportion, $x+a : x-a :: 2\sqrt{ax} : b$; and hence, $\frac{x-a}{x+a}\sqrt{ax} = \frac{1}{2}b$. But it is here manifeſt, that if a be taken for the tangent of an arc, of which the radius is $\sqrt{ax}$, x will be the cotangent of it, and $\frac{x-a}{x+a}\sqrt{ax}(=\frac{1}{2}b)$ the coſine of twice that arc. Conſequently, we have to find an arc, the tangent of which is to the coſine of twice that arc as a is to $\frac{1}{2}b$; and this being done, the natural co-tangent of that arc, to the proper radius, will be the value of x.

Thus, let a be 10, and b 24; and the difference of the logarithms of a and $\frac{1}{2}b$ will be 0.0791812. Now, by running the eye along GARDINER's Tables of logarithmic Sines and Tangents, it will be readily ſeen, that the log. tangent of 26° 33′ 50″, when increaſed by 0.0791812, is leſs than the coſine of 53° 7′ 40″ twice that arc; and that the log. tangent of 26° 34′ 0″, when increaſed by the ſame quantity, is too great. And, by actually taking out the logarithms, and making the additions, the former will be found too ſmall by 455, and the latter too great by 632. Then, 1087 (455 + 632) : 10″ :: 455 : 4″; which being added to 26° 33′ 50″ gives 26° 33′ 54″ for the arc of which x is the co-tangent. And if to twice the log. co-tangent of this arc the logarithm of a (10) be added, the ſum (1.6020600) is the log. of 40, the value of x ſought.

EXAMPLE VI.

The equation reſulting from a ſolution of the famous problem of ALHAZEN may be given as another example of the uſe of this method. Many ſolutions of this celebrated problem, by HUYGENS, SLUSIUS, and others, may be met with in the Philoſophical Tranſactions. Solutions to it may alſo be found at the end of Dr. ROBRRT SIMPSON's Conic Sections, in Dr. SMITH's Optics, Mr. ROBIN's Mathematical Tracts, and other places; but the moſt direct and obvious method is, perhaps, that which follows.

Put $a = DC$, $b = dC$, $r = CI$; $x = CB$ and $y = CE$, the coſines of the arcs IA, IH, to the radius r: then will the ſines of thoſe arcs, BA, EH, be expreſſed by $\sqrt{r^2 - x^2}$ and $\sqrt{r^2 - y^2}$;

and,

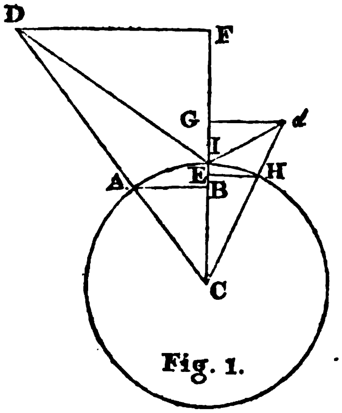

Fig. 1.

and, becauſe of the ſimilar triangles ABC and BFC, HEC and *d*GC, $r : x :: a : \frac{ax}{r} =$ CE, $r : \sqrt{r^2 - x^2} :: a : \frac{a\sqrt{r^2 - x^2}}{r} =$ DF; $r : y :: b : \frac{by}{r} =$ CG, and $r : \sqrt{r^2 - y^2} :: b : \frac{b\sqrt{r^2 - y^2}}{r} =$ *d*G; conſequently, $\frac{ax}{r} - r =$ FI, $\frac{bx}{r} - r =$ GI; and becauſe the angles DIF, *d*IG, are equal by the nature of the problem, and the angles DFI and *d*GI both right angles, the triangles DFI and *d*GI are alſo ſimilar, and conſequently $\frac{a\sqrt{r^2 - x^2}}{r} : \frac{ax}{r} - r :: \frac{b\sqrt{r^2 - y^2}}{r} : \frac{by}{r} - r$; and $\frac{x}{\sqrt{r^2 - x^2}} - \frac{r^2}{a\sqrt{r^2 - x^2}} = \frac{y}{\sqrt{r^2 - y^2}} - \frac{r^2}{b\sqrt{r^2 - y^2}}$; or $\frac{r^2}{b\sqrt{r^2 - y^2}} - \frac{r^2}{a\sqrt{r^2 - x^2}} = \frac{y}{\sqrt{r^2 - y^2}} - \frac{x}{\sqrt{r^2 - x^2}}$: or, the co-ſecant of the arc HI $\div b$ – co-ſecant of AI $\div a =$ the co-tangent HI $\div r$ – co-tangent AI $\div r$; or, laſtly, the co-tangent of HI – co-tangent of AI = co-ſecant of HI × $\frac{r}{b}$ – co-ſecant AI × $\frac{r}{a}$. Conſequently, we have to find two arcs, the ſum of which is given, and ſuch that the difference of their co-tangents may be equal to the difference of the products of their co-ſecants into given quantities.

To do this aſſume the angle DCF as near as poſſible; and, becauſe the ſum of the two angles is given, the angle *d*CG will be known alſo. Take the difference of the logarithms of r and a, r and b, which will be conſtant, alſo the difference of the co-tangents of the two aſſumed arcs, and having taken out the log. co-ſecants, add to them reſpectively the two logarithmic differences. Find the numbers correſponding to theſe two ſums, and if the difference of theſe two numbers be equal to the difference of the co-tangents, the angle DCF was rightly aſſumed; but as that will ſeldom happen, take the difference, or error; aſſume the angle DCF again, repeat the operation, and find the error as before. Then, as the ſum of the errors, if one of them was too great, and the other too little, or their difference, if both were too great, or both too little, is to the difference of theſe aſſumptions, ſo is the leſs error to a number of minutes and ſeconds, which muſt be added to that aſſumption to which the leaſt error belongs, if that aſſumption was too ſmall; or ſubtracted from it, if the aſſumption was too great: and, unleſs the firſt aſſumption was made very wide of the truth, which may always be avoided, the two angles will generally be obtained within a few ſeconds of the truth, and, by repeating the operation once more, to the utmoſt exactneſs.

Suppoſe DC (a) be taken equal to 72, dC (b) = 48, and the radius CI (r) = 40, the angle DCd being 82° 45′: then the whole operation will ſtand as follows:

$r = 40$ log.	11.6020600	- -	11.6020600
$a = 72$ log.	1.8573325	$b = 48$ log.	1.6812412
Conſtant log.	9.7446275	Conſtant log.	9.9208188

Now, in the two triangles DCI, dCI, the angles DIC and dIC being equal, and CI common, but dC conſiderably leſs than DC, it is manifeſt, that the angle dCI will be conſiderably leſs than the angle DCI: let them be aſſumed in the proportion that DC bears to its exceſs above dC; in which caſe the angle dCI will be 27° 35′ and DCI 55° 10′. The co-tangent of the former will be 1.9141795, of the latter .6958813; and the difference of them 1.2182982. The log. co-ſecants of thoſe two angles are 10.3343832 and 10.0857536, which being reſpectively increaſed by 9.9208188 and 9.7446275, the two conſtant logarithms, make 0.2552020 and 9.8304811, which are the logarithms of 1.7997079 and .6768323; and the difference of theſe two numbers is 1.1228756, which is leſs than the difference of the log. co-tangents by .0954226.

I next aſſume the angles 30° and 52° 45′; and by purſuing the ſame ſteps which have been deſcribed above, I find the difference of their co-tangents exceeds the difference of the products by .0028987. Then, as 925239 (the difference of the errors) is to 145′ (the difference of ſuppoſitions), ſo is the latter error 28987 to 4′ 33″, which being added to 30°, gives 30° 4′ 33″ for the next aſſumption of the angle *d*CI; but for eaſe in the computation I ſhall take 30° 5′; in which caſe the angle DCI will be 52° 40′; and by repeating the operation the difference of the co-tangents will be found leſs than the difference of the products by .0002425. And 31412 (the ſum of the two laſt errors) is to 5′ (the difference of the ſuppoſitions) as 2425 (the laſt error) is to 23″; which being taken from 30° 5′, the laſt ſuppoſition, becauſe it was too great, leaves 30° 4′ 37″ for the exact value of the angle *d*CI.

This equation, like that in the fourth example, when the value of *y* is properly ſubſtituted, and the equation reduced in the uſual manner, will riſe to four dimenſions with all the inferior ones; and it does not appear, that either HUYGENS, SLUSIUS, Mr. ROBINS, Dr. WILLSON, or Profeſſor SIMSON, with all their artifice, have been able to depreſs it: but by this method of reſolution the point of reflection is found, with the greateſt exactneſs, in much leſs time than this ſubſtitution and reduction can be made. And this example farther ſuggeſts to us, that when the anſwer is ſought by the method now under conſideration, it is not always neceſſary to exterminate all the unknown quantities but one.

EXAMPLE VII.

Suppoſe the equation to be reſolved were $a = .375 = 16y^5 \mp 4y^4 - 20y^3 \pm 4y^2 + 5y$: and, firſt, let the upper ſigns have place, and it is manifeſt, that the latter ſide of the equation may be divided into two parts; namely, $4y^2 - 4y^4 = 4y^2 \cdot \overline{1+y} \cdot \overline{1-y}$, and $16y^5 - 20y^3 + 5y = y^5 - 10y^3 \cdot \overline{1+y} \cdot \overline{1-y} + 5y \cdot \overline{1+y}|^2 \cdot \overline{1-y}|^2$. But the former part is (by tab. *I.*) the ſquare of the ſine of twice the arc which has y for its ſine (radius being $= 1$) and the latter part the ſine of five times the ſame arc. Hence, therefore, the given quantity a $(= .375)$ is equal to the ſum of the ſine of five times the arc (A) which has y for its ſine, and the ſquare of the ſine of twice the ſame arc. Now, as the ſquare of the ſine of twice the arc (A) muſt neceſſarily in this inſtance be very ſmall in compariſon of the ſine of five times the ſame arc (A), it is manifeſt, that the ſine of five times the arc which has y for its ſine will be very little leſs than .375, and of courſe that arc (5A) can be but very little leſs than 22° 2′, the ſine of which is next greater than that number. Aſſume it 21°, and the fifth part of it, or that arc which has y for its ſine, will be 4° 12′, the double of which is 8° 24′. Now the log. ſine of 8° 24′ is 9.1645998, which being doubled is 8.3291996, the logarithm of .0213403, and this number being taken from .375 leaves .3536597, which ought to have been .3583679, the ſine of 21°, and of courſe is too ſmall by .0047082: the arc has, therefore, been aſſumed too great.

Let 20° 45′ be next aſſumed; the fifth part of which is 4° 9′, and twice this laſt number is 8° 18′, of which the log. ſine is 9.1594354; and this being doubled is 8.3188708, the log. of .0208387; and this being taken from .375 will leave .3541613: leſs ſtill than the ſine of 20° 45′ by .0001297.

Take now 20° 44′, the fifth of which is 4° 8′ 48″, and two-fifths is 8° 17′ 36″; and the log. ſine of this is 9.1590889, which being doubled gives 8.3181778, the logarithm of .0208055; and this being taken from .375, leaves .3541945; more than the ſine 20° 44′ by .0000795. Now 2092 (the ſum of the laſt two laſt errors) is to 60″ as 795 (the laſt error) is to 23″. Which being added to 20° 44′, the laſt aſſumption, gives 20° 44′ 23″ for five times the arc of which *y* is the ſine: *y* is therefore the ſine of 4° 8′ 52″.6, or .07233202.

When the lower ſigns in the equation have place, the given quantity *a* will be equal to the exceſs of the ſine of five times an arc above the ſquare of the ſine of twice that arc: and the operation, after aſſuming, from the circumſtances of the queſtion, or equation, an arc which is nearly five times that having *y* for its ſine, is this. Find the logarithmic ſine of two-fifths of that arc, double it, find the number correſponding to this logarithm, and add to it the value of *a*, which ſhould then be equal to the ſine of the arc firſt aſſumed; and if it is not, to repeat the operation until an error is obtained on each ſide, and not very diſtant from the truth, as is done above, and which may always be done with three aſſumptions.

A multitude of examples might be added from the writings of different authors, who have either left their concluſions unexhibited in numbers, for want of ſome ſuch eaſy method as this, or have done it by means of a long and laborious ſeries of difficult computations; which, beſide the labour attending them, are always ſubject to a variety of errors, which cannot be detected, in many caſes, without repeating the operation.

XXXI. *Experiments on the Power that Animals, when placed in certain Circumſtances, poſſeſs of producing Cold. By* Adair Crawford, *M. D.; communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read June 14, 1781.

IN the following paper I ſhall lay before the Society the reſult of ſome experiments, which I made in the courſe of the laſt ſummer, on the power that animals, when placed in certain circumſtances, poſſeſs, of producing cold, having premiſed a few remarks on the progreſſive improvements which have been made in the knowledge of heat in general.

The opinions of the ancients, reſpecting the nature and properties of fire, conſiſted of bold conjectures, which ſeem rather to have been the offspring of a lively and vigorous imagination, than of a juſt and correct judgement: their ideas on this ſubject being evidently derived, not ſo much from an accurate obſervation of facts, as from thoſe ſentiments of admiration and awe which many of the phenomena of fire are calculated to excite. Thus, this element was ſuppoſed, on the original formation of the univerſe, to have aſcended to the higheſt place, and to have occupied the region of the heavens: it was conceived to be the principle which firſt communicated life and activity to the animal kingdom: it was conſidered as conſtituting the eſſence of inferior intellectual beings; and, by many of the ancient nations, it was reverenced as the ſupreme Deity. Indeed the profound veneration with which the element of fire was contemplated, for a long ſucceſſion of ages, by a great part of mankind, appears to be one of the moſt curious circumſtances in the hiſtory of antient opinions. To account for this we may obſerve, that there is no principle in nature, obvious to the ſenſes, which produces ſuch important effects in the material ſyſtem, and which, at the ſame time, in the mode of its operation, is ſo obſcure and incomprehenſible.

It appears to be accumulated in an immenſe quantity in the ſun and fixed ſtars, from whence its beneficial influence ſeems to be continually diffuſed over the univerſe: it is the great inſtrument by means of which the changes of the ſeaſons are effected; the diverſity of climates is chiefly owing to the various proportions in which it is diſtributed throughout the earth. If we add to this the mighty alterations which have been produced in human affairs by the introduction of artificial fire, by its employment in the ſeparation of metals from their ores, and in the various arts which are ſubſervient to the comfort, the ornament, and the preſervation, of the ſpecies, it will not appear ſurprizing, that in a rude and ignorant age, this wonderful principle ſhould have been conſidered as endued with life and intelligence, and that it ſhould have become the object of religious veneration.

In the dark ages the alchymiſts regarded pure fire as the reſidence of the Deity: they conceived it to be uncreated and immenſe, and attributed to its influence moſt of the phenomena of nature. Indeed, it is not wonderful that they ſhould have aſſigned it a high rank in the ſcale of being, as it was the great agent which they employed in the chymical analyſis of bodies, and was the inſtrument of thoſe diſcoveries that attracted ſuch univerſal admiration, and that enabled them ſo ſucceſſfully to impoſe upon the ignorance and credulity of the times.

Upon the revival of literature, the importance of this branch of ſcience began very ſoon to engage the attention of philoſophers. It could not eſcape the general obſervation, in a penetrating and inquiſitive age, when the powers of the human mind were employed with ſo much ardour and ſucceſs in exploring the operations of nature, that the element of fire acts a principal part in the ſyſtem of the world; that by the influence of this element thoſe motions are begun and ſupported in the animal and vegetable kingdoms, which are eſſential to the production and preſervation of life; and that it is the great agent in thoſe ſucceſſive combinations and decompoſitions, by which all things on the ſurface of the earth, and probably throughout the univerſe, are kept in a continual fluctuation.

But though the utility of this branch of ſcience was perceived, yet the progreſs that was made in the cultivation of it did not keep pace with the opinion which men entertained of its importance. Our ſenſes inform, us, that heat has a real exiſtence, but they give us no direct information with regard to its nature and properties: it is endowed with ſuch infinite ſubtilty, that it has been called, by a very eminent philoſopher, an occult quality: by ſome it has even been conſidered as an immaterial being. It is, therefore, with great difficulty that it can be made the ſubject of philoſophical inveſtigation; and hence the opinions of men concerning it have been fluctuating and various, and the words which expreſs it vague and ambiguous.

The firſt ſtep that was taken with a view to the cultivation of this branch of ſcience was the conſtruction of a machine for meaſuring the variations of ſenſible heat; obſerving, that heat has the power of expanding bodies, and conſidering the degree of expanſion as proportional to the increaſe of heat; philoſophers have endeavoured by means of the former to render the latter obvious to the ſenſes.

To this important invention, the author of which cannot be diſtinctly traced, we are indebted for all the ſucceeding improvements in the philoſophy of heat. By means of it men were enabled to eſtabliſh a variety of intereſting facts, and to bring ſome of the moſt obſcure and intricate phenomena of nature to the teſt of experiment. The opinion, that the heats inherent in various heterogeneous ſubſtances differed from each other in kind, as well as in degree, was now exploded, ſince all were found to produce ſimilar effects upon the thermometer. The increaſe and diminution of temperature in the different ſeaſons and climates, the laws which nature obſerves in the heating and cooling of bodies, the melting, the vaporific, and ſhining points, and the degrees of heat in the animal, the mineral, and the vegetable kingdoms, were accurately determined. In conſequence of the attention that was paid to this ſubject, many curious queſtions aroſe, which have long exerciſed the ingenuity of philoſophers. That property of heat by which it is capable of expanding the denſeſt and hardeſt bodies; its power in producing fluidity; its tendency to an equilibrium; and the cauſes of its various diſtribution throughout the different ſubſtances in nature, have become the objects of philoſophical enquiry. It was obſerved, that ſome bodies on expoſure to heat, become red and luminous, but are incapable of producing flame, or of maintaining fire: that, on the contrary, others, by the application of fire, and the contact of freſh air, kindle into flame, and continue to emit light and heat, apparently from a ſource within themſelves, till they are conſumed. Hence aroſe the queſtions concerning the pabulum ſum of fire, the uſe of the air in inflammation, and the diſtinction of bodies into combuſtible and incombuſtible.

From the firſt dawnings of philoſophy it muſt have been perceived, that moſt animals have a higher temperature than the medium in which they live; and that a conſtant ſucceſſion of freſh air is neceſſary to the ſupport of animal life. The cauſes of theſe phenomena have afforded matter for much ſpeculation in ancient as well as modern times: but the diſcovery that animals have, in certain circumſtances, the power of keeping themſelves at a lower temperature than the ſurrounding medium, was reſerved for the induſtry of the preſent age.

This diſcovery ſeems originally to have ariſen from obſervations on the heat of the human body in warm climates. It was mentioned by Governor ELLIS in 1758; it was taught by Dr. CULLEN before the year 1765; and at length it was completely eſtabliſhed by the experiments of Dr. FORDYCE in heated rooms, which were laid before the Society in 1774.

In the courſe of theſe experiments the doctor remained in a moiſt air heated to 130° for the ſpace of fifteen minutes, during which time the thermometer under his tongue ſtood at 100°, his pulſe made 139 beats in a minute, his reſpiration was but little affected, and ſtreams of water ran down over his whole body, proceeding from the condenſation of vapour, as evidently appeared from a ſimilar condenſation on the ſide of a Florentine flaſk that had been filled with water at 100°.

He found, however, that he could bear a much greater degree of heat when the air was dry. In this ſituation, he frequently ſupported, naked, for a conſiderable time, without much inconvenience, the heat of 260°, his body preſerving very nearly its proper temperature, being never raiſed more than 2° above the natural ſtandard.

Various opinions have been entertained with regard to the cauſes of the facts which were eſtabliſhed by theſe experiments. Some have attributed the cold ſolely to evaporation, and have conceived that the ſame degree of refrigeration would have been produced by an equal maſs of dead matter, containing an equal quantity of moiſture. Others have affirmed, that the cold did not ariſe ſolely from this cauſe; but have maintained, that it depended partly upon the energy of the vital principle, being greater than what would have been produced by an equal maſs of inanimate matter.

The ingenious Dr. MUNRO, of Edinburgh, aſcribes the cold in the above mentioned experiments to the circulation of the blood, in conſequence of which the warmer fluids are continually propelled from the ſurface towards the center, where they are mixed with blood at a lower temperature, and hence the animal is ſlowly heated, in the ſame manner as the water in a deep lake, during the winter, is ſlowly cooled, and not without a long continuance of froſt congealed, no part of it becoming ſolid till the whole is brought down to the freezing point.

The following experiments were made with a view to determine with greater certainty the cauſes of the refrigeration in the above inſtances.

To diſcover whether the cold produced by a living animal, placed in air hotter than its body, be not greater than what would be produced by an equal maſs of inanimate matter, I took a living and a dead frog, equally moiſt, and of nearly the ſame bulk, the former of which was at 67°, the latter at 68°, and laid them upon flannel in air which had been raiſed to 106°. In the courſe of twenty-five minutes the order of heating was as follows *.

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* In the two following experiments the thermometers were placed in contact with the ſkin of the animals under the axillæ.

	Air.	Dead frog.	Living frog.
′	°	°	°
In 1	—	70½	67½
2	102	72	68
3	100	72½	69½
4	100	73	70
25	95	81¼	78¼

The thermometer being introduced into the ſtomach, the internal heat of the animals was found to be the ſame with that at the ſurface.

From hence it appears, that the living frog acquired heat more ſlowly than the dead one. Its vital powers muſt, therefore, have been active in the generation of cold.

To determine whether the cold produced in this inſtance depended ſolely upon the evaporation from the ſurface, increaſed by the energy of the vital principle, a living and dead frog were taken at 75°, and were immerſed in water at 93°, the living frog being placed in ſuch a ſituation as not to interrupt reſpiration.

	Dead frog.	Living frog.
′	°	°
In 1	85	81
2	88½	85
3	90½	87
5	91½	89
6	91½	89
8	91½	89

Theſe experiments prove, that living frogs have the faculty of reſiſting heat, or producing cold, when immerſed in warm water: and the experiments of Dr. FORDYCE prove, that the human body has the ſame power in a moiſt as well as in a dry

* In the above experiment the water, by the cold frogs and by the agitation which it ſuffered during their immerſion, was reduced nearly to 91°½.

air: it is therefore highly probable, that this power does not depend ſolely upon evaporation.

It may not be improper here to obſerve, that healthy frogs, in an atmoſphere above 70°, keep themſelves at a lower temperature than the external air, but are warmer internally than at the ſurface of their bodies: for when the air was 77°, a frog was found to be 68°, the thermometer being placed in contact with the ſkin; but when the thermometer was introduced into the ſtomach, it roſe to 70°½.

It may likewiſe be proper to mention, that an animal of the ſame ſpecies placed in water at 61°, was found to be nearly 61°¼ at the ſurface, and internally it was 66°½. Theſe obſervations are meant to extend only to frogs living in air or water at the common temperature of the atmoſphere in ſummer. They do not hold with reſpect to thoſe animals, when plunged ſuddenly into a warm medium, as in the preceding experiments.

To determine whether other animals alſo have the power of producing cold, when ſurrounded with water above the ſtandard of their natural heat, a dog at 102° was immerſed in water at 114°, the thermometer being cloſely applied to the ſkin under the axilla, and ſo much of his head being uncovered as to allow him a free reſpiration.

In 5 minutes the dog was 108°, water 112°
6 - - 109 - 112
11 - - 108 - 112 the reſpiration having become very rapid.

In thirteen minutes the dog was 108°, water 112°, the reſpiration being ſtill more rapid.

In about half an hour the dog was 109°, water 112°; the animal was then in a very languid ſtate.

Small quantities of blood being drawn from the femoral artery, and from a contiguous vein, the temperature did not ſeem to be much increaſed above the natural ſtandard, and the ſenſible heat of the former appeared to be nearly the ſame with that of the latter.

In this experiment a remarkable change was produced in the appearance of the venous blood: for it is well known, that in the natural ſtate, the colour of the venous blood is a dark red, that of the arterial being light and florid; but after the animal, in the experiment in queſtion, had been immerſed in warm water for half an hour, the venous blood aſſumed very nearly the hue of the arterial, and reſembled it ſo much in appearance, that it was difficult to diſtinguiſh between them. It is proper to obſerve, that the animal which was the ſubject of this experiment, had been previouſly weakened by loſing a conſiderable quantity of blood a few days before. When the experiment was repeated with dogs which had not ſuffered a ſimilar evacuation, the change in the colour of the venous blood was more gradual; but in every inſtance in which the trial was made, and it was repeated ſix times, the alteration was ſo remarkable, that the blood which was taken in the warm bath could readily be diſtinguiſhed from that which had been taken from the ſame vein before immerſion, by thoſe who were unacquainted with the motives or circumſtances of the experiment.

To diſcover whether a ſimilar change would be produced in the colour of the venous blood in hot air, a dog at 102° was placed in air at 134°.

In ten minutes the temperature of the dog was 104°½, that of the air being 130. In fifteen minutes the dog was 106°, the air 130°. A ſmall quantity of blood was then taken from the jugular vein, the colour of which was ſenſibly altered, being much lighter than in the natural ſtate.

The effect which is produced by external heat upon the colour of the venous blood, ſeems to confirm the following opinion, which was firſt ſuggeſted by my worthy and ingenious friend Mr. WILSON, of Glaſgow. Admitting that the ſenſible heat of animals depends upon the ſeparation of abſolute heat from the blood by means of its union with the phlogiſtic principle in the minute veſſels, may there not be a certain temperature at which that fluid is no longer capable of combining with phlogiſton, and at which it muſt of courſe ceaſe to give off heat? It was partly with a view to inveſtigate the truth of this opinion that I was led to make the experiments recited above.

I ſhall now endeavour, from the preceding facts, to explain what appear to me to be the true cauſes of the cold produced by animals when placed in a medium, the temperature of which is above the ſtandard of their natural heat.

In a work which I ſome time ago laid before the public, having attempted to prove, that animal heat depends upon the ſeparation of elementary fire from the air in the proceſs of reſpiration, I obſerved, that when an animal is placed in a warm medium, if the evaporation from the lungs be increaſed to a certain degree, the whole of the heat ſeparated from the air will be abſorbed by the aqueous vapour.

From the experiments on venous and arterial blood, recited in the third ſection of that work, it appears, that the capacity of the blood for containing heat is ſo much augmented in the lungs, that, if its temperature were not ſupported by the heat which is ſeparated from the air, in the proceſs of reſpiration, it would ſink 30°. Hence, if the evaporation from the lungs be ſo much increaſed as to carry off the whole of the heat that is detached from the air, the arterial blood when it returns by the pulmonary vein will have its ſenſible heat greatly diminiſhed, and will conſequently abſorb heat from the veſſels which are in contact with it, and from the parts adjacent. The heat which is thus abſorbed in the greater veſſels will again be extricated in the capillaries, where the blood receives a freſh addition of phlogiſton. If, in theſe circumſtances, the blood during each revolution were to be equally impregnated with this latter principle, it is manifeſt, that the whole effect of the above proceſs would be to cool the ſyſtem at the center, and to heat it at the ſurface; or to convey the heat to that part of the body where it is capable of being inſtantly carried off by evaporation. But it appears, from the experiments which have been laſt recited, that, when an animal is placed in a heated medium, the ſanguineous maſs, during each revolution, is *leſs* impregnated with phlogiſton; for we have ſeen, that the venous blood, in theſe circumſtances, becomes gradually paler and paler in its colour till at length it acquires very nearly the appearance of the arterial: and it is rendered highly probable by the experiments of Dr. PRIESTLEY, that the dark and livid colour of the blood in the veins depends upon its combination with phlogiſton in the minute veſſels. Since, therefore, in a heated medium, this fluid does not aſſume the ſame livid hue, we may conclude, that it does not attract an equal quantity of the phlogiſtic principle *.

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* It is of no conſequence in the above argument, whether we ſuppoſe, with Dr. PRIESTLEY, that the alteration of colour in the blood depends upon its combination with phlogiſton in the capillary arteries, or maintain with ſome other philoſophers that this alteration ariſes from a change produced in the blood itſelf by the action of the veſſels; it is ſufficient for our purpoſe to aſſume it as a fact, which, I think, has been proved by direct experiment, that, in the natural ſtate of the animal, the blood undergoes a change in the capillaries, by which its capacity for containing heat diminiſhed; and that in a heated medium it does not undergo a ſimilar change.

It follows, that the quantity of heat given off by the blood in the capillaries will not be equal to that which it had abſorbed in the greater veſſels, or poſitive cold will be produced. If the blood, for example, in its paſſage to the capillaries, abſorb from the greater veſſels a quantity of heat as 30°, and if in conſequence of its receiving a leſs impregnation of phlogiſton than formerly, it give off at the extreme veſſels a quantity of heat only as 20°, it is manifeſt, that upon the whole a degree of refrigeration will be produced as 10°, and this cauſe of refrigeration will continue to act while the venous blood is gradually aſſuming the hue of the arterial, till the difference between them is obliterated; after which it will ceaſe to operate. Thus it appears, that when animals are placed in a warm medium, the ſame proceſs which formerly ſupplied them with heat becomes for a time the inſtrument of producing cold, and probably preſerves them from ſuch rapid alterations of temperature as might be fatal to life.

Upon the whole, the increaſed evaporation, the diminution of that power by which the blood in the natural ſtate is impregnated with phlogiſton, and the conſtant reflux of the heated fluids towards the internal parts, ſeem to be the great cauſes upon which the refrigeration depends. Having found that the attraction of the blood to phlogiſton was diminiſhed by heat, it appeared probable, on the other hand, that it would be increaſed by cold. To determine this, a dog at 100° was immerſed in water nearly at 45°. In about a quarter of an hour a ſmall quantity of blood was taken from the jugular vein, which was evidently much deeper in its colour than that which had been taken in the warm bath, and appeared to me, as well as to ſeveral other gentlemen, to be the darkeſt venous blood we had ever ſeen.

From this experiment, compared with thoſe which have been recited above, we may perceive the reaſon why animals preſerve an equal temperature, notwithſtanding the great variations in the heat of the atmoſphere, ariſing from the viciſſitudes of the weather, and the difference of ſeaſon and climate: for, as ſoon as by expoſure to external cold, an unuſual diſſipation of the vital heat is produced, the blood, in the courſe of the circulation, begins to be more deeply impregnated with the phlogiſtic principle. It will therefore furniſh a more copious ſupply of this principle to the air in the lungs, and will imbibe a greater quantity of fire in return.

In ſummer, on the contrary, the reverſe of this will take place, leſs phlogiſton will be attracted in the minute veſſels, and leſs fire will be abſorbed from the air.

And hence the power of generating heat is in all caſes proportioned to the demand. It is increaſed by the winter colds, diminiſhed by the ſummer heats: it is totally ſuſpended or converted into a contrary power, according as the exigencies of the animal may require.

From the changes which are produced in the colour of the venous blood by heat and cold, we may likewiſe perceive the reaſon why the temperature of the body is frequently increaſed by plunging ſuddenly into cold water, and why the warm bath has ſuch powerful effects in cooling the ſyſtem, and in removing a general or partial tendency to inflammation.

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XXXII. *Account of a Comet. By Mr.* Herſchel, *F. R. S.*; *communicated by Dr.* Watſon, *Jun. of* Bath, *F. R. S.*

Read April 26, 1781.

ON Tueſday the 13th of March, between ten and eleven in the evening, while I was examining the ſmall ſtars in the neighbourhood of H Geminorum, I perceived one that appeared viſibly larger than the reſt: being ſtruck with its uncommon magnitude, I compared it to H Geminorum and the ſmall ſtar in the quartile between Auriga and Gemini, and finding it ſo much larger than either of them, ſuſpected it to be a comet.

I was then engaged in a ſeries of obſervations on the parallax of the fixed ſtars, which I hope ſoon to have the honour of laying before the Royal Society; and thoſe obſervations requiring very high powers, I had ready at hand the ſeveral magnifiers of 227, 460, 932, 1536, 2010, &c. all which I have ſucceſsfully uſed upon that occaſion. The power I had on when I firſt ſaw the comet was 227. From experience I knew that the diameters of the fixed ſtars are not proportionally magnified with higher powers, as the planets are; therefore I now put on the powers of 460 and 932, and found the diameter of the comet increaſed in proportion to the power, as it ought to be, on a ſuppoſition of its not being a fixed ſtar, while the diameters of the ſtars to which I compared it were not increaſed in the ſame ratio. Moreover, the comet being magnified much beyond what its light would admit of, appeared hazy and ill-defined with theſe great powers, while the ſtars preſerved that luſtre and diſtinctneſs which from many thouſand obſervations I knew they would retain. The ſequel has ſhewn that my ſurmiſes were well founded, this proving to be the Comet we have lately obſerved.

I have reduced all my obſervations upon this Comet to the following tables. The firſt contains the meaſures of the gradual increaſe of the Comet's diameter. The micrometers I uſed, when every circumſtance is favourable, will meaſure extremely ſmall angles, ſuch as do not exceed a few ſeconds, true to 6, 8, or 10 thirds at moſt; and in the worſt ſituations true to 20 or 30 thirds: I have therefore given the meaſures of the Comet's diameter in ſeconds and thirds. And the parts of my micrometer being thus reduced, I have alſo given all the reſt of the meaſures in the ſame manner; though in large diſtances, ſuch as one, two, or three minutes, ſo great an exactneſs, for ſeveral reaſons, is not pretended to.

TABLE I. Meaſures of the Comet's diameter *.

Days.		″	‴	Powers.	
March	17	2	53	932. 460.	
	19	2	59	932. 460.	
	21	3	38	460.	
	28	4	7	932	these meaſures agree to 9‴.
	—	3	58	227	
	29	4	7	227	rather too ſmall a meaſure.
	—	4	25	227	ſeems right.
April	2	4	25	227	
	6	4	53	227	
	15	5	11	227	
	—	5	20	227	very good; not liable to half a ſecond of error.
	18	5	2.	227	true to 12‴ or 18‴ at moſt.

Having meaſured the diameter of the Comet with ſuch high power as 932 and 460, it may not be amiſs to make one obſervation on this ſubject, leſt it ſhould be miſapprehended that I pretend to a diſtinct power of ſuch magnitude upon all celeſtial objects in general. By experience I have found, that the aberration or indiſtinctneſs occaſioned by magnifying much, provided the object be ſtill left ſufficiently diſtinct, is rather to be put up with, than the power to be reduced, when the angles to be meaſured are extremely ſmall. The reaſon of this may, perhaps, be that a ſmall error of judgement, to which we are always liable, is of great conſequence with a low power, as bearing a conſiderable proportion to the diameter of the object;

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* There are ſeveral optical deceptions which may affect the meaſures of objects that ſubtend extremely ſmall angles. Thus I have found, by experience, that a very ſmall object will appear ſomething leſs in a teleſcope when we ſee it firſt than when we become familiar with it. There is alſo a deflection of light upon the wires when they are nearly ſhut; but as none of theſe deceptions are well enough underſtood to apply a correction, I leave them affected with them.

whereas with a higher power the proportion of this error to the whole becomes much leſs, and the meaſure more exact, even after we have made allowance for a ſmall additional error occaſioned by the want of that perfect diſtinctneſs which is required for other purpoſes. However, to enter deeply into an explanation of this would lead me to ſpeak of the cauſes of the aberration of rays in the focus of an object ſpeculum, of which there are ſome that are ſeldom taken into conſideration by opticians, and indeed are ſuch as cannot be calculated; but this not being my preſent purpoſe, ſuffice it to obſerve, that the method is juſtified by experience.

When the diameter of the Comet was increaſed to about 4″, I thought it adviſable to leſſen the power with which I meaſured; and, as I made uſe of two different micrometers, as well as eye-glaſſes, I took a meaſure with both of them. The agreement of the micrometers to 9‴ is no ſmall proof of the goodneſs of the obſervations of the 28th of March, and very properly connects the meaſures of the high powers with thoſe that were made with 227.

TABLE II. Diſtance of the Comet from certain teleſcopic fixed ſtars which I have marked α, β, γ, δ, ε, ζ.

D.	H.	M.		′	″	‴	
Mar. 13	10	30	from α,	2	48	0	by pretty exact eſtimation true to 20″.
17	11	0	fig. 1*.	0	41	58	by the micrometer and power 227.
18	7	20		1	0	35	
—	9	16		1	6	59	
—	10	55		1	10	40	
19	7	4		1	46	40	
—	10	42		1	51	23	
21	10	0		3	39	46	
24	8	12	from β,	2	55	39	true to 4 or 5″, an indifferent obſervation.
—	10	58	fig. 2.	2	53	4	true to 4 or 5″.
25	7	24		2	12	46	true to 2 or 3″.
—	9	47		2	14	18	
26	10	43		1	48	3	true to 2 or 3″.
28	7	46		2	55	49	true to 4 or 5″.
29	8	50	from γ.	2	20	51	true to 2″.
30	7	55	fig. 3.	1	28	48	true to 2 or 3″.
Apr. 1	7	45		2	39	20	
6	8	50	from δ. fig. 4.	2	51	25	
15	10	18	from ε.	4	27	57	eſtimated by the field, true to 5 or 6″.
16	7	50	fig. 5.	3	9	14	by the micrometer, true to 3 or 4″.
—	10	47		2	50	56	true to 3 or 4″.
18	8	18		3	18	4	mean 3′ 17″, true to 1″ or 1½.
—	—	—		3	15	57	
—	8	50	from ζ,	2	24	57	
19	8	38	fig. 6.	3	2	5	true to 3 or 4″.

* The figures are drawn upon a ſcale of 80 ſeconds to one inch.

TABLE III. Angle of poſition of the Comet with regard to the parallel of declination of the ſame teleſcopic fixed ſtars meaſured by a micrometer, of which I have given the deſcription, and a magnifying power of 278. See fig. 1. 2. 3. 4. 5. 6.

	D.	H.	M.		°	′		
Mar.	13	10	30	B α Comet,	0	0	by ſuperficial eſtimation, liable to an error of 10 or 12 degrees.	
	17	11	0	A α Comet,	89	56	by the micrometer.	
	18	8	20	fig. 1 *.	56	39		
	—	9	24		41	33	true to 1°.	
	19	7	23		29	47	true to 1°.	
	21	10	10		11	46	true to 4 or 5°.	
	—	11	48		12	14		
	24	8	23	B β Comet,	38	39	true to 2 or 3°.	
	—	11	4	fig. 2.	36	14	true to 3 or 4°, air very tremulous.	
	25	7	33		53	18		
	—	9	55		56	32	liable to a conſiderable error.	
	26	10	55	A β Comet.	87	0	true to 2 or 3°.	
	28	7	58		28	51	true to 3 or 4°.	
	29	9	25	B γ Comet,	32	19	true to 1 or 2°.	
	30	8	25	fig. 3.	72	14	true to 3 or 4°.	
Apr.	1	7	55	A γ Comet,	28	51	well taken,	27° 46′, true to 1°.
	—	—	—		27	14	more exact,	
	6	8	28	B δ Comet, fig. 4.	84	42	true to leſs than 2°	
	15	10	27	B ε Comet,	29	9	true to 2 or 3°.	
	16	8	1	fig. 5.	49	11	true to 1°.	
	—	10	55		50	47	true to 1½ or 2°½.	
	18	8	31	A ε Comet,	47	9	very well taken,	47°, true to leſs than 1°.
	—	—	—		46	35	pretty well,	
	—	9	18	B ζ Comet,	82	39		
	19	8	56	A ζ Comet,	48	18		49° 3′, true to 1°.
	—	—	—	fig. 6.	49	48		
	—	10	45		47	30	true to 2 or 3°.	

* The angles are drawn true to the meaſure, without allowing for errors.

Miſcellaneous obſervations and remarks.

March 19. The Comet's apparent motion is at preſent $2\frac{1}{4}$ ſeconds *per* hour. It moves according to the order of the ſigns, and its orbit declines but very little from the ecliptic.

March 25. The apparent motion of the Comet is accelerating, and its apparent diameter ſeems to be increaſing.

March 28. The diameter is certainly increaſed, from which we may conclude that the Comet approaches to us.

April 2. This evening at 8 h. 15′ the Comet was a *little* above the line drawn from η to θ in fig. 7. This figure is *only* delineated by the eye, ſo that no very great exactneſs in the diſtances of the ſtars is to be expected; but I ſhall take the firſt opportunity of meaſuring their reſpective ſituations by the micrometer.

April 6. With a magnifying power of 278 times the Comet appeared perfectly ſharp upon the edges, and extremely well defined, without the leaſt appearance of any beard or tail.

April 16. Fig. 8. repreſents the ſituation of the Comet this evening about nine o'clock, and is only an eye-draught of the teleſcopic ſtars.

Remarks on the path of the Comet.

We may obſerve, that the method of tracing out the path of a celeſtial body by taking its diſtance from certain ſtars, and the angle of poſition, with regard to them, cannot be expected to give us a compleatly juſt repreſentation of the tract it deſcribes, ſince even the moſt careful obſervations are liable to little errors, both from the remaining imperfections of inſtruments, though they ſhould be the moſt accurate that can be had, and from the difficulty of taking angles and poſitions of objects in motion. Add to this a third cauſe of error, namely, the obſcurity of very ſmall teleſcopic ſtars that will not permit the field of view ſo well to be enlightened as we could wiſh, in order to ſee the threads of the micrometer perfectly diſtinct.

This will account for the apparent diſtortions to be obſerved in my figures of the Comet's path. Some little irregularity therein may alſo proceed from different refractions, as they have not been taken into account, though the obſervations have been made at very different altitudes, where conſequently the refractions muſt have been very different. But though this method may be liable to great inconveniences, the principal of which is, that many parts of the heavens are not ſufficiently ſtored with ſmall ſtars to give us an opportunity to meaſure from them, yet the advantages are not leſs remarkable. Thus we ſee that it enabled me to diſtinguiſh the quantity and direction of the motion of this Comet in a ſingle day (from the 18th to the 19th of March) to a much greater degree of exactneſs than could have been done in ſo ſhort a time by a ſector or tranſit inſtrument; nay even an hour or two, we ſee, were intervals long enough to ſhew that it was a moving body, and conſequently, had its ſize not pointed it out as a Comet, the change of place, though ſo trifling as 2¼ ſeconds *per* hour, would have been ſufficient to occaſion the diſcovery. A gentleman very well known for his remarkable ſucceſs in detecting Comets * ſeems to be well aware of the difficulty to diſcover a motion in a heavenly body by the common methods when it is ſo very ſmall; for in a letter he favoured me with, ſpeaking of the Comet, he ſays: "Rien n'etoit plus difficile que de la "reconnoître et je ne puis pas concevoir comment vous avés pu

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* Monſ. MESSIER.

" revenir pluſieurs fois ſur cette étoile ou Comête; car abſolu-
" ment il a fallu l'obſerver pluſieurs jours de ſuite pour s'ap-
" perçevoir qu'elle avoit un mouvement."

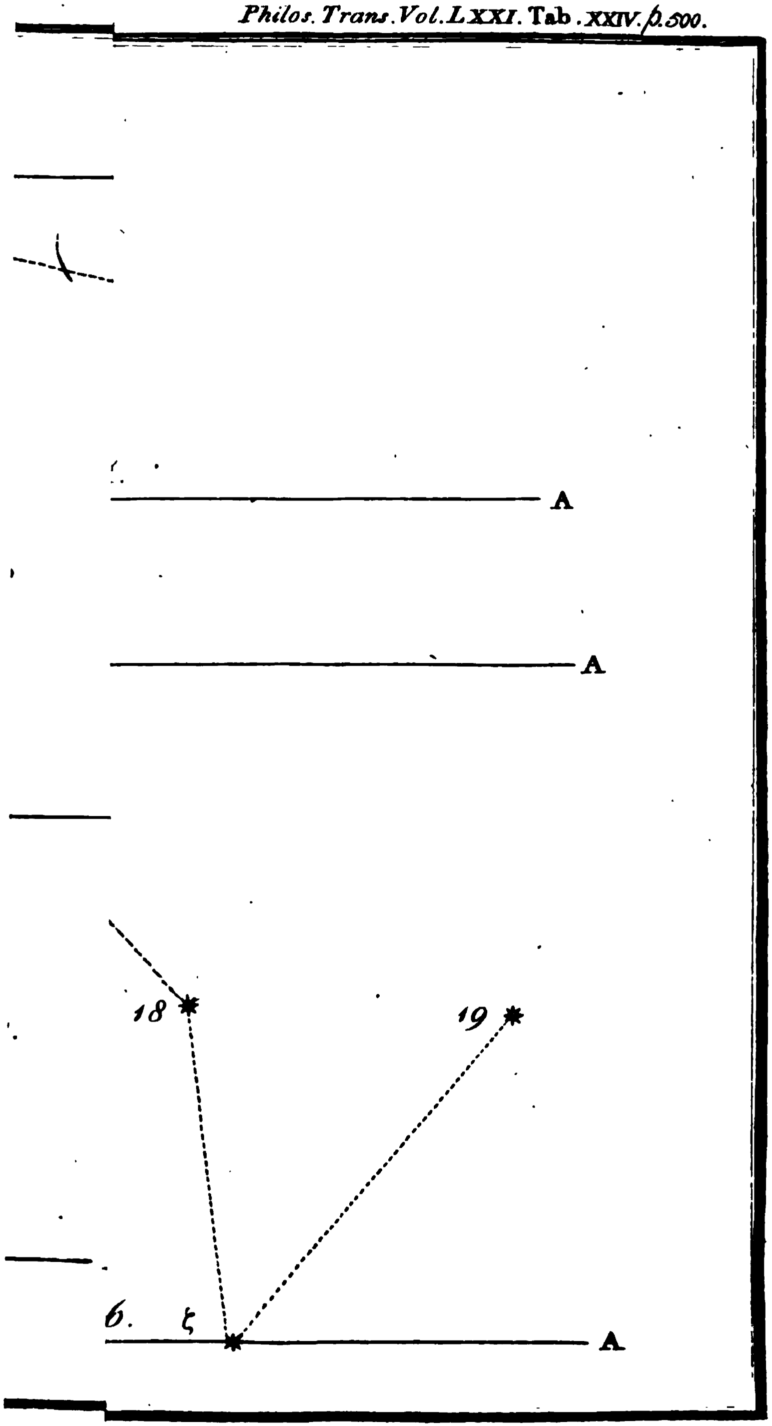
I need not ſay that I merely point this out as a temporary advantage in the method I have taken; for as ſoon as we can have regular, conſtant, and long continued obſervations by fixed inſtruments, the excellence of them is too well known to ſay any thing upon that ſubject: for which reaſon I failed not to give immediate notice of this moving ſtar, and was happy to ſurrender it to the care of the Aſtronomer Royal and others, as ſoon as I found they had begun their obſervations upon it.

Deſcription of a micrometer for taking the angle of poſition.

FIG. I. Repreſents the micrometer incloſed in a turned caſe of wood, as it is put together, ready to be uſed with the teleſcope. A is a little box which holds the eye-glaſs. B is the piece which covers the inſide work, and the box A is ſcrewed into it. C is the body of the micrometer containing the braſs work, ſhewing the index plate *a* projecting at one ſide, where the caſe is cut away to receive it. D is a piece, having a ſcrew *b* at the bottom, by means of which the micrometer is faſtened to the teleſcope. To the piece C is given a circular motion, in the manner the horizontal motion is generally given to Gregorian reflectors, by the lower part going through the piece D, where it is held by the ſcrew E, which keeps the two pieces C and D together, but leaves them at liberty to turn upon each other.

Fig. II. Is a ſection of the caſe containing the braſs work, where may be obſerved the piece B hollowed out to receive the

Philos. Trans. Vol. LXXI. Tab. XXIV. p. 500.



Basire sc.

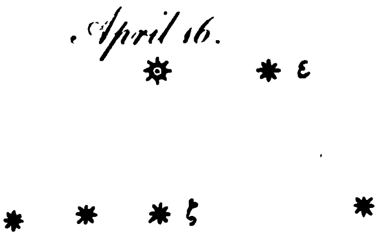


Fig. 8.

Basire. Sc.

Fig. II.

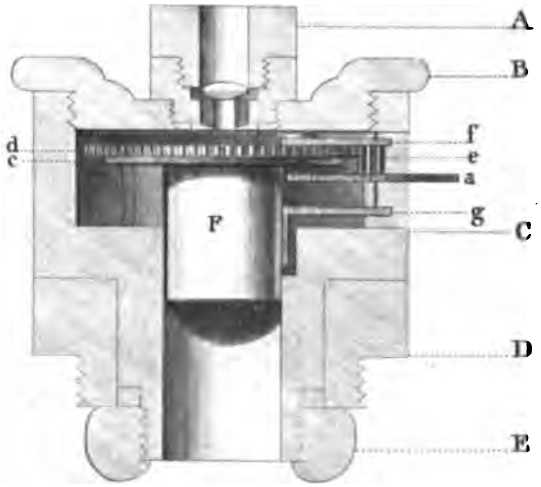
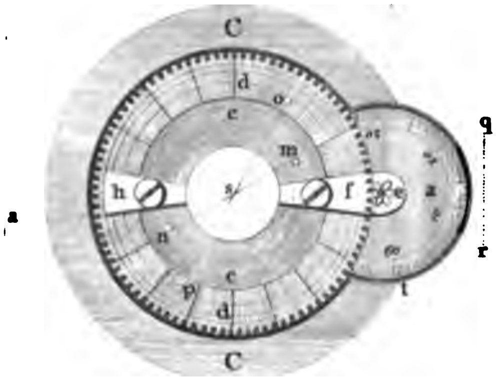


Fig. IV.



BASIRE sc.

box A, which conſiſts of two parts incloſing the eye lens. This figure alſo ſhews how the piece C paſſes through D, and is held by the ring E: the braſs work, conſiſting of a hollow cylinder, a wheel and pinion, and index plate, is there repreſented in its place. F is the body of the braſs work, being a hollow cylinder with a broad rim *c* at the upper end; this rim is partly turned away to make a bed for the wheel *d*. The pinion *e* turns the wheel *d*, and carries the index plate *a*. One of its pivots moves in the arm *f*, ſcrewed upon the upper part of *c*, which arm ſerves alſo to confine the wheel *d* to its place upon *c*. The other pivot is held by the arm *g* faſtened to F.

Fig. III. Is a plan of the braſs work. The wheel *d*, which is in the form of a ring, is laid upon the upper part of F or *c*, and held by two ſmall arms *f* tnd *h*, ſcrewed down to *e* with the ſcrews *i*, *i*.

Fig. IV. Is a plan of the braſs work. *d*, *d*, is the wheel placed upon the bed or ſocket of the rim of the cylinder *c*, *c*, and is held down by the two pieces *f*, *h*, which are ſcrewed upon *e*, *e*. The piece *f* projects over the center of the index plate to receive the upper pivot of the pinion *m*, *n*, is the fixed wire faſtened to *c*, *c*. *o*, *p*, the moveable wire faſtened to the annular wheel *d*, *d*. The index plate *a* is divided into 60 parts, each ſub-divided into two, and milled upon the edge. When the finger is drawn over the milled edge of the index plate from *q* towards *r*, the angle *m*, *s*, *o*, will open, and if drawn from *r* towards *q*, it will ſhut again. The caſe C, C, muſt have a ſharp corner *t*, which ſerves as a hand to poin out the diviſion on the index plate.

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XXXIII. *A Letter from* Joſeph Willard *to the Rev. Dr.* Maſkelyne, *Aſtronomer Royal, concerning the Longitude of* Cambridge *in* New England.

Read July 5, 1781.

REV. SIR,

THE difference of meridians between Greenwich and Cambridge has been generally reckoned 4 h. 44′. This was what the late Dr. WINTHROP made uſe of; but I do not find that he determined it by actual obſervations, made by him at Cambridge, compared with correſponding ones, made at the Royal Obſervatory at Greenwich. It appears, that in 1769, at the time of the tranſit of Venus, the doctor was not entirely certain of the longitude of Cambridge. He mentioned 4 h. 44′ as near the truth; but for better fixing it, he gave ſeveral of his obſervations of the eclipſes of Jupiter's ſatellites to be compared with thoſe made at Greenwich; but there were too few correſponding ones to determine the point with preciſion; and as modern aſtronomers do not make abſolute dependence upon the difference of meridians deduced from the eclipſes of Jupiter's ſatellites, unleſs there has been a ſeries of obſervations, both of immerſions and emerſions, I have wiſhed to find ſome obſervations of ſolar eclipſes and occultations of fixed ſtars by the moon, made at Cambridge, of which correſponding ones were made at Greenwich. I have met with no obſervations of occultations made by Dr. WINTHROP; but a ſolar eclipſe was obſerved by him and ſeveral other gentlemen, at his houſe, Auguſt 5, 1766, at which I was preſent and aſſiſting, being then a reſident graduate at Harvard College: this eclipſe, Sir, I find that you obſerved at Greenwich. By your obſervations in the printed volume, a copy of which the Royal Society was ſo generous as to ſend to Harvard College, which was received the laſt ſpring, and for which the College is very grateful, I find the beginning of the eclipſe was ſeen by you at 5 h. 29′ 56″ P.M. and the end at 7 h. 11′ 27″ P.M. apparent time. At Dr. WINTHROP's houſe at Cambridge, lat. 42° 25′ N. the beginning of this eclipſe was obſerved at 11 h. 39′ 23″ A.M. and the end at 2 h. 45′ 9″ P.M. apparent time. Allowing for the ſpheroidal figure of the earth, and going through the parallactic calculations and deductions, I find the difference of meridians between Greenwich and Cambridge, by the obſervations of this eclipſe, to be 4 h. 44′ 22″.

In the tranſit of Venus, in 1769, the internal contact was obſerved by Dr. WINTHROP at 2 h, 47′ 30″ apparent time, and by Mr. HITCHINS, at the Royal Obſervatory, at 7 h. 28′ 57″ apparent time. Allowing the ſun's parallax on the day of the tranſit to be 8″,38, I find by calculation from theſe obſervations, that the difference of meridians between Greenwich and Cambridge is 4 h. 44′ 12″. The reaſon of my taking Mr. HITCHINS's obſervation is, your ſaying, that the teleſcope which he uſed was much ſuperior to all the others which were made uſe of at that day at the Obſervatory; and to its greater excellence and diſtinctneſs you attribute the difference of 26″ by which Mr. HITCHINS ſaw the internal contact before you. There ſeems to be the greater propriety, when comparing the American obſervations of that phenomenon with thoſe made at Greenwich, to take that where the obſerver was peculiarly aided by the diſtinctneſs of his teleſcope, becauſe the ſun was very near the horizon with you, while with us the altitude was great, and the atmoſphere exceeding clear. Taking the mean between the deduction made from the obſervations of the internal contact of Venus, and of the beginning and ending of the above ſolar eclipſe, the difference of meridians between Greenwich and Cambridge is 4 h. 44′ 17″.

I find, Sir, in a letter from you to Dr. SMITH of Philadelphia, Dec. 26, 1769, that by the obſervations of the eclipſes of Jupiter's ſatellites made at Norriton you determined the difference of meridians between Greenwich and Norriton to be 5 h. 1′ 0′ 35″. If we ſubtract 52″, the difference of meridians between Philadelphia and Norriton, gotten, agreeably to your requeſt, by terreſtrial meaſurement, we find the difference of meridians between Greenwich and Philadelphia to be 5 h. 0′ 43″, which is the ſame that it appears to be by the immerſions and emerſions of Jupiter's firſt ſatellite obſerved at Philadelphia, corrected in the ſame manner you corrected the obſervations for Norriton, which is 8″ more than Dr. EWING's determination. By obſervations of the tranſit of Mercury in 1769, made at Cambridge and Philadelphia, the difference of meridians between thoſe two places appears by the external contact to be 16′ 02″, by the internal 16′ 28″; the mean 16′ 30″ ſubtracted from 5 h. 0′ 43″ leaves 4 h. 44′ 13″ for the difference of meridians between Greenwich and Cambridge deduced in this way, which, though not direct, may yet be conſidered as an evidence of ſome weight to prove, that the difference is more than 4 h. 44′, and that 4 h. 44′ 17″ may be very near the truth. This is the difference that I at preſent take, when I make uſe of tables fitted to the meridian of Greenwich; but I ſhould be ſtill glad of more correſponding obſervations to aſcertain this point. June 24, 1778, there was a ſolar eclipſe, viſible both at Greenwich and Cambridge. The beginning of this eclipſe was obſerved at Cambridge by the late Dr. WINTHROP at 9 h. 6′ 20″ A.M. and the end at 11 h. 37′ 22″ A.M. apparent time. If the atmoſphere favoured your obſerving it at Greenwich, I ſhould be extremely obliged to you, if you would communicate to me your obſervations. I ſhall alſo be happy to know the time of the beginning of the ſolar eclipſe of the 23d of next April at Greenwich.

I now, Sir, beg leave to communicate to you ſome obſervations of the ſolar eclipſe of the 27th of laſt October. At Beverly, lat. 42° 36′ N. I carefully aſcertained the going of my clock, by equal altitudes of the ſun's upper and lower limb, for ſeveral days preceding the eclipſe, and on the day when it happened, conſtantly applying the equation for the change of declination. October 25th, when the ſun's center paſſed the meridian, it was by the clock 11 h. 59′ 11″; on the 26th, 11 h. 58′ 15″; and on the 27th, 11 h. 57′ 18″.

Two gentlemen obſerved with me; the rev. Mr. CUTLER of Ipſwich, and the rev. Mr. PRINCE of Salem. Mr. CUTLER and I were each furniſhed with a reflecting teleſcope made by JAMES MANN of London; one magnifying 34, and the other 45 times. Mr. PRINCE had an achromatic refractor magnifying 43 times. The times of obſervation are as follow:

		By the clock.			Reduced to app. time.			
		h.	′	″	h.	′	″	
Beginning of eclipſe obſerved by	Mr. CUTLER,	10	59	2	11	1	42	A.M.
	Mr. PRINCE,	10	59	6	11	1	46	
	Myſelf,	10	59	8	11	1	48	
End of eclipſe obſerved by	Mr. CUTLER,	1	38	37	1	41	23	P.M.
	Mr. PRINCE,	1	38	43	1	41	29	
	Myſelf,	1	38	40	1	41	26	

I had no micrometer to meaſure the lucid part of the ſun in its leaſt ſtate, and thereby determine the error of MAYER's tables in latitude at the middle of this eclipſe. But according to the obſervations of Profeſſor WILLIAMS, Dr. WINTHROP's ſucceſſor, the error was not great. He and ſeveral aſſiſtants obſerved this eclipſe at Long Iſland in Penobſcot Bay. The latitude of the place of obſervation he found to be 44° 17′ 7″ N. He obſerved the beginning of the eclipſe at 11 h. 11′ 8″ A.M. and the end at 1 h. 50′ 25″ P.M. apparent time. He was furniſhed with an excellent DOLLOND's micrometer, with which he meaſured the ſun's diameter on the morning of the eclipſe, and the lucid parts many times during its continuance. By his obſervations, compared with thoſe made at Beverly, I find the difference of meridians between Beverly and Long Iſland to 8′ 4″ in time. The time of the greateſt obſcuration was at 12 h. 30′ 22″, when the lucid part of the ſun was 8″ on the lower limb. The ſun's ſemi-diameter according to obſervation was 16′ 8″,7; the moon's horizontal ſemi-diameter, according to the tables, 16′ 23″,8; the augmentation of her ſemi-diameter, agreeably to her altitude, 9″,5; the ſum of the viſible ſemi-diameters of the ſun and moon therefore 32′ 42″. The lucid part of the ſun 8″ being ſubtracted from 32′ 17″,4, the ſun's diameter, leaves 32′ 9″,4 for the eclipſed part of the ſun, which ſubtracted from 32′ 42″ leaves 32″,6 for the leaſt diſtance of the centers of the ſun and moon. The viſible angle of the moon with the ſun was, I find, 15° 54′ 54″, and her motion from the ſun in her viſible orbit 24″,5 in one minute; the viſible ecliptic conjunction therefore was at 12 h. 29′ 57″, and the diſtance of the centers of the ſun and moon 34″. The moon's parallax in latitude from the ſun was then 49′ 15,″8 S. which added to 34″ gives 49′ 49″,8 N. equal to the moon's latitude

tude by obſervation. By MAYER's tables her latitude was then 49′ 39″,4, by which it appears, that the error of theſe tables in latitude, at the middle of the eclipſe, by the Penobſcot obſervation, was – 10″4. The error in longitude, taking the mean between that of the beginning and end, I find to be – 9″. The determination of theſe errors is upon the ſuppoſition that Beverly is 4 h. 42′ 59″ W. of Greenwich, which, I believe, very near the truth. Beverly appears by obſervation to be 1′ 18″ in time E. of Cambridge, which ſubtracted from 4 h. 44′ 17″ leaves 4 h. 42′ 59″.

I hope, Sir, no umbrage will be taken at my writing to you, on account of the political light in which America is now viewed by Great Britain. I think political diſputes ſhould not prevent communications in matters of mere ſcience; nor can I ſee how any one can be injured by ſuch an intercourſe.

I am, &c.

Beverly in Maſſachuſetts,
February 16, 1781.

AN

ACCOUNT

OF SOME

THERMOMETRICAL EXPERIMENTS;

CONTAINING,

I. Experiments relating to the cold produced by the evaporation of various fluids, with a method of purifying ether.
II. Experiments relating to the expanſion of mercury.
III. Deſcription of a thermometrical barometer.

By TIBERIUS CAVALLO, F.R.S.

Who was nominated by the President and Council to proſecute Diſcoveries in Natural Hiſtory, purſuant to the Will of the late HENRY BAKER, Eſq. F.R.S.

Read at the ROYAL SOCIETY, June 28, 1781,

AN ACCOUNT, &c.

IT is at preſent well known, that by the evaporation of various fluids a ſenſible degree of cold is produced; and that by the evaporation of ether, which is the moſt volatile fluid we are acquainted with, water may be congealed, and the thermometer may be brought ſeveral degrees below the freezing point. But as various thermometrical experiments, which I lately made, have exhibited ſome new phenomena, and as I have contrived an eaſy and pleaſing method of freezing a ſmall quantity of water in a ſhort time, and in every climate; I think it not improper to give an account of theſe things in the firſt part of this lecture.

My firſt experiments were intended to diſcover, if poſſible, a fluid cheaper than ether, by the evaporation of which a degree of cold ſufficient for ſome uſeful purpoſe might be generated. But in this my expectation was diſappointed, as I found that ether was incomparably ſuperior to any other fluid, as the cold it produced was ſeveral degrees greater than that occaſioned by any other of the moſt volatile fluids whatever. Being, therefore, obliged to uſe ether, I endeavoured to contrive a method, by which the leaſt poſſible quantity of it might be waſted in the production of a degree of cold ſufficient to freeze water, and in this I met with ſucceſs. But before we come to the deſcription of this method, I ſhall briefly relate ſome obſervations made on the cold produced by the evaporation of other fluids beſides ether.

In a room, the temperature of which was 64° according to FAHRENHEIT's thermometer, and in which the air was gently ventilated, I obſerved the effects produced by various fluids when thrown upon the ball of a thermometer. The ball of this thermometer was quite detached from the ivory piece upon which the ſcale was engraved. The various fluids were thrown upon the thermometer through the capillary aperture of a ſmall glaſs veſſel, ſhaped like a funnel, and care was taken to throw them ſo ſlowly upon the bulb of the thermometer, that a drop might now and then fall from the under part of it; except when thoſe fluids were uſed, which evaporate very ſlowly, in which caſe it was ſufficient to keep the ball of the thermometer only moiſt, without any drop falling from it. During the experiment the thermometer was kept turning very gently round its axis, in order that the fluid uſed might fall upon every part of its bulb. This method I find to anſwer much better than that of dipping the ball of the thermometer into the fluid and removing it immediately after, or that of wetting the thermometer with a feather. The evaporation, and conſequently the cold produced by it, may be increaſed by ventilation. *viz.* by blowing with a pair of bellows upon the thermometer; but this was not uſed in the following experiments, becauſe it is not eaſily performed by one perſon, and alſo becauſe it occaſions very uncertain reſults.

With the above deſcribed method I began to examine the effects of water, and found, that the thermometer was brought down to 56°, *viz.* 8° below the temperature of the room in which the experiment was made, and of the water employed.

This effect was produced in about two minutes time, after which a larger continuation did not bring the mercury lower.

By means of ſpirit of wine the thermometer was brought down to 48°, which is only 16° below the temperature of the room, and of the ſpirit employed. When the ſpirit of wine is highly rectified, the cold produced by its evaporation is certainly greater than when it is of the common ſort; but the difference is not ſo great as one, who never tried the experiment, might expect. The purer ſpirit produces the effect much quicker.

Uſing various other fluids, which were either compounds of water and ſpiritous ſubſtances, or pure eſſences, I found that the cold produced by their evaporation was generally in ſome intermediate degree between the cold produced by the water and that produced by the ſpirit of wine.

Spirit of turpentine brought the thermometer only 3° lower than the temperature of the room; but olive oil and other oils, which evaporate either very ſlowly or not at all, did not ſenſibly affect the thermometer.

Willing to obſerve how much electrization could increaſe the evaporation of ſpirit of wine, and conſequently the cold produced by it, I put the tube containing the ſpirit into an inſulating handle, and connected it with the conductor of an electrical machine, which was kept in action whilſt the experiment was performed; by theſe means the thermometer was brought down to 47°. Having tried the three mineral acids I found, that inſtead of cooling they heated the thermometer, which effect I expected; ſince it is well known, that thoſe acids attract the water from the atmoſphere, and that heat is produced by the combination of water and any of them. The vitriolic acid, which was very ſtrong and tranſparent, raiſed the thermometer to 102°; the ſmoaking nitrous acid raiſed it to 72°; and the marine acid raiſed it to 66°; the temperature of the room, as well as of the acids, being 64°, as mentioned above.

The apparatus which I contrived for the purpoſe of uſing the leaſt poſſible quantity of ether in freezing water, &c. conſiſts in a glaſs tube, terminating in a capillary aperture, which tube is to be fixed upon the bottle that contains the ether. Fig. 1. of the annexed drawing exhibits ſuch a tube, round the lower part of which, *viz.* at A ſome thread is wound, in order to let it fit the neck of the bottle. When the experiment is to be made, the ſtopper of the bottle containing the ether is removed, and the above mentioned tube is fixed upon it. The thread round this tube ſhould be moiſtened a little with water or ſpittle before it is fixed on the bottle, in order to prevent more effectually any eſcape of ether between the neck of the bottle and the tube. Then holding the bottle by its bottom FG (fig. 2.) and keeping it inclined as is ſhewn in the figure, the ſmall ſtream of ether iſſuing out of the aperture D of the tube DE, is directed upon the ball of the thermometer, or upon a tube containing water or other liquor that is required to be congealed.

Ether being very volatile, and having the remarkable property of increaſing the bulk of air, does not require any aperture, through which the air might enter the bottle, in proportion as the ether goes out: the heat of the hand is more than ſufficient to force the ether in a ſtream from the aperture D.

After this manner, throwing the ſtream of ether upon the ball of a thermometer in ſuch quantity as that a drop of ether might now and then, for inſtance every 10 ſeconds, fall from the under part of the thermometer, I have brought the mercury down to 3°, *viz.* 29° below the freezing point, when the atmoſphere was ſomewhat hotter than temperate, and that without blowing upon the thermometer.

When the ether is very good, *viz.* is capable of diſſolving elaſtic gum, and the thermometer has a ſmall bulb, not above twenty drops of ether are required to produce this effect, and about two minutes of time; but when the ether is of the common ſort, a greater quantity of it, and a longer time, is neceſſary to be employed, though at laſt the thermometer is brought down very nearly as low by this as by the beſt ſort of ether.

In order to freeze water by the evaporation of ether, I take a thin glaſs tube about four inches long and about one-fifth of an inch in diameter, hermetically cloſed at one end, and put a little water in it, ſo as to fill about half an inch length of it, as is ſhewn at CB in the figure. Into this tube a ſlender wire H is alſo introduced, the lower extremity of which is twiſted in a ſpiral manner, and ſerves to draw up the ice, when formed. Things being thus prepared, I hold the glaſs tube by its upper part A with the fingers of the left hand, and keep it continually and gently turning round its axis, firſt one way, and then the contrary; whilſt with the right hand I hold the phial containing the ether in ſuch a manner as to direct the ſtream of ether on the outſide of the tube, and a little above the ſurface of the water in it. The capillary aperture D ſhould be kept almoſt in contact with the ſurface of the tube that contains the water. Continuing this operation for two or three minutes, the water will be froze as it were in an inſtant; ſince it will appear to become opaque at the bottom B, and the opacity will aſcend at C in leſs than half a ſecond of time, which exhibits a beautiful appearance. This congelation, however, is only ſuperficial, and in order to congeal the whole quantity of water,

ter, the operation muſt be continued one or two minutes longer; after which the wire H will be found to be kept very tight by the ice. Now the bottle with the ether is left upon a table or other place, and to the outſide of the glaſs tube the hand muſt be applied for a moment, in order to ſoften the ſurface of the ice, which adheres very firmly to the glaſs, and then pulling the wire H out of the tube, a ſolid and hard piece of ice will come out, faſtened to its ſpiral extremity.

Inſtead of the wire H ſometimes I put a ſmall thermometer into this tube ſo as to have its bulb immerſed in the water. With this thermometer I have obſerved a very remarkable phenomenon, which ſeems to be not explicable in the preſent ſtate of knowledge concerning heat and cold. This is, that water will freeze in the winter with a leſs degree of cold than it will in the ſummer, or when the weather is hotter: for inſtance, in the winter the water in the tube AB will freeze when the thermometer is about 30°; but in the ſummer, or even when the temperature of the atmoſphere is about 60°, the quickſilver in the thermomer muſt be brought ten or fifteen, or even more, degrees below the freezing point, before the water which ſurrounds the ſaid thermometer will be converted into ice, even ſuperficially; hence it appears, that in the ſummer time a greater quantity of ether and longer time is required to freez a given quantity of water than in the winter, not only becauſe then a greater degree of heat is to be overcome, but principally becauſe in the ſummer a much greater degree of cold muſt be actually produced before the water that is kept in it will aſſume a ſolid form. When the temperature of the atmoſphere has been about 40°, I have froze a quantity of water with an equal weight of good ether, but at preſent, being ſummer, between two and three times the quantity of the ſame ether muſt be uſed to produce the ſame effect.

There ſeems to be ſomething in the air which, beſides heat, interferes with the freezing of water, and perhaps of all fluids, though I cannot ſay from experience whether the above mentioned difference between the freezing of water in winter and ſummer, takes place with other fluids, as milk and other animal fluids, oils, wines, &c.

The proportion between the quantity of the ether and of the water that may be frozen by it, ſeems to vary according to the quantity of water; for a larger quantity of water ſeems to require a proportionably leſs quantity of ether than a ſmaller quantity of water, ſuppoſing that the water is contained in cylindrical glaſs veſſels; for I have not tried whether a metal veſſel inſtead of a glaſs one, and whether ſome other ſhape beſides the cylindrical, might not facilitate the congelation. In the beginning of the ſpring I froze about a quarter of an ounce of water with nearly half an ounce weight of ether, the apparatus being larger, though ſimilar to that deſcribed above.

Now as the price of ether, ſufficiently good for the purpoſe, is generally between eighteen pence and two ſhillings *per* ounce, it is plain, that with leſs than two ſhillings a quarter of an ounce of ice, or ice cream, may be made in every climate, and at any time, which may afford great ſatisfaction to thoſe perſons, who living in places where no natural ice is to be had, never ſaw or taſted any ſuch delicious refreſhments.

When a ſmall piece of ice, for inſtance, of about ten grains in weight, is wanted, the neceſſary apparatus is very ſmall, and the expence of the ether not worth mentioning. I have a ſmall box, which is four inches and a half long, two inches broad, and one inch and a half deep, which contains all the apparatus.

ratus neceſſary for this purpoſe, *viz.* a bottle capable of containing about one ounce of ether, two pointed tubes (in caſe that one ſhould break) a tube in which the water is to be frozen, and the wire. With the quantity of ether contained in this ſmall and very portable apparatus, the experiment, when carefully performed, may be repeated about ten times. A perſon who wiſhes to perform ſuch experiments in hot climates, and in places where ice is not eaſily procured, requires only a large bottle of ether, beſides the ſmall apparatus deſcribed above.

It is a known fact, that the moment a quantity of water becomes ice, a thermometer kept immerſed in it, riſes a few degrees, and accordingly this is obſerved in our experiment, *viz.* the mercury of the thermometer, which is immerſed in the water of the tube AB, will ſuddenly riſe, ſometimes as much as ten degrees, when the water becomes firſt opaque. Electrization increaſes very little the degree of cold produced by the evaporation of ether. Having thrown the electrified, and alſo the unelectrified, ſtream of ether upon the bulb of a thermometer, the mercury in it was brought down two degrees lower in the former than in the latter caſe.

As various perſons may, perhaps, be induced by this paper to repeat ſuch experiments, and as ether is a fluid which can with difficulty be preſerved, it may be uſeful to mention, that a cork confines ether in a glaſs bottle much better than a glaſs ſtopple, which it is almoſt impoſſible to grind ſo well as intirely to prevent the evaporation of ether. When a ſtopple, made very nicely out of a uniform and cloſe piece of cork, which goes rather tight, is put upon a bottle of ether, the ſmell of that fluid cannot be perceived through it; but I never ſaw a glaſs ſtopple that could produce the ſame effect. By opening

ing the bottle very often, or by long keeping, the cork becomes looſe, in which caſe it muſt be changed; and thus ether, ſpirit of wine, or any fluid, excepting thoſe which corrode cork, may be preſerved.

I ſhall now deſcribe a method of purifying vitriolic ether, which is very eaſy and expeditious, though not very profitable: this method I learned of Mr. WINCH, Chemiſt, in the Hay-market. Fill about a quarter of a ſtrong bottle with common ether, and upon it pour about twice as much water, then ſtop the bottle, and give it a ſhake, ſo as to mix for a time the ether with the water. This done, keep the bottle without motion, and with the mouth downwards, till the ether is ſeparated from the water and ſwims over it, which requires not above three or four minutes of time; then open the bottle, and keeping it ſtill inverted, let the greateſt part of the water come out very gently; after this the bottle being turned with the mouth upwards, more water muſt be poured in it, and in ſhort the ſame operation muſt be repeated three or four times. Laſtly, all the water being ſeparated from the ether by decanting it with dexterity, the ether will be found to be exceedingly pure. By this means I have purified common vitriolic ether, which could not affect elaſtic gum, and have reduced it into ſuch a ſtate as that elaſtic gum was eaſily diſſolved by it. Indeed this purified ether appeared by every trial to be purer than I ever ſaw it, even when made after the beſt uſual method, and in the moſt careful manner. The only inconvenience attending this proceſs is, that a vaſt quantity of ether is loſt. Not above three or four ounces of a pound of common ether remain after the purification. As the greateſt part of the ether is certainly mixed with the water that is uſed in the proceſs, it may perhaps be worth while to put that water into a retort, and to diſtil the ether from it, which muſt come ſufficiently pure for common uſe.

It is commonly believed, that water combines with the pureſt part of ether, when thoſe two fluids are kept together; whereas, by the above deſcribed proceſs, the contrary is eſtabliſhed: perhaps when ether is kept in contact with water for a long time, the pureſt part of it may appear to be loſt, becauſe the ether may be combined with, and may retain ſome water in itſelf, at the ſame time that the water combines with and retains ſome ether, whereas the caſe may be different when the ether is quickly waſhed in water, and is immediately after ſeparated from it: but in reſpect to this I have yet not made any experiments, ſo as to be able to decide the matter.

II. *Experiments relating to the expanſion of mercury.*

THE difficulty and uncertainty attending the various methods hitherto propoſed for inveſtigating the expanſion of quickſilver, or its increaſe of bulk when rarified by a given degree of heat, determined me to contrive ſome method by which this purpoſe might be effected with more certainty and preciſion. After various experiments I hit upon the following method, which to me ſeems both new and capable of great accuracy, though in this I may be deceived.

Firſt, having blown a ball to a capillary tube, ſuch as are commonly uſed for thermometers, I weighed it, and found that this empty thermometer was equal to 79,25 grains. This empty glaſs previous to its being weighed was rendered as perfectly clean as poſſible, which is a neceſſary precaution in this experiment, which depends upon a very great accuracy of weight. Then I introduced ſome mercury into the ſtem of this thermometer, taking care that none of it entered the ball, and, by adapting a ſcale of inches to the tube, obſerved that 4,3 inches length of the tube was filled with the mercury. The thermometer was now weighed again, and from this weight, the weight of the glaſs found before being ſubtracted, the remainder, *viz.* 0,24 gr. ſhewed the weight of ſo much quickſilver as filled 4,3 inches of the tube. Now the ball of the thermometer, and alſo part of the tube, were intirely filled with quickſilver: then, in order to find out the weight of the mercury contained in it, the thermometer was weighed for the laſt time, and from this weight the weight of the glaſs being ſubtracted, the remainder, *viz.* 3205 grs. ſhewed the weight of the whole quantity of quickſilver contained in the thermometer.

By compariſon with a graduated thermometer in hot and cold water, I made a ſcale to the new thermometer according to FAHRENHEIT's, and by applying a ſcale of inches found, that the length of 20° in this ſcale was equal to 1,33 inch. But 0,24 gr. was the weight of ſo much mercury as filled 4,3 inches length of the tube; therefore, by the rule of proportion it will be found, that the weight of ſo much quickſilver as fills 1,33 inch of the tube, *viz.* the length of 20° is equal to 0,0742 gr. nearly, and that the weight of ſo much quickſilver as fills the length of the tube that is equivalent to one degree, is equal to 0,00371 gr. Now it is clear, that the weight of the whole quantity of quickſilver contained in the thermometer is to the weight of ſo much quickſilver as fills the length of one degree in the tube, as the bulk of the whole quantity of quickſilver in a given degree of heat, to the increaſe of bulk, that the ſame whole quantity of quickſilver acquires when heated of but 1°; *viz.* 32,05 grs. is to 0,00371 gr. as 1 is to 0,0011 +; ſo that by this experiment it appears, that 1° of FAHRENHEIT's thermometer increaſes the bulk of mercury not above $\frac{11}{100000}$ths parts. In this proceſs a ſmall deviation from mathematical exactneſs is occaſioned by the ſmall difference of weight between the quickſilver of the tube when firſt weighed and when it is afterwards heated to 1°; but by an eaſy calculation it will be found, that this difference is ſo exceedingly ſmall as not to be perceived by our exacteſt weighing and meaſuring inſtruments.

For clearneſs ſake I ſhall ſubjoin the calculation of the above related experiments, diſencumbered from words. Here the decimals are not computed to a very large number, that being unneceſſary for this purpoſe.

Weight of the glaſs, - - - -	79,25 grs.
Weight of ſo much quickſilver as filled 4,3 inches length of the tube, - - -	0,24 grs.
Weight of the whole quantity of quickſilver contained in the thermometer, - -	32,05 grs.
Length of the tube equal to 20°, - -	1,33 inch.

$4{,}3 : 0{,}24 :: 1{,}33 : 0{,}0742 = 20°$

$20° : 0{,}0742 :: 1 : 0{,}00371$

$32{,}05 : 0{,}00371 :: 1 : 0{,}00011 +$ = to the expanſion occaſioned by one degree of heat.

Having repeated this experiment with other thermometers, and by ſimilar calculations, each proceſs gave a reſult little different from the others, which irregularity is certainly owing to the imperfection of my ſcales, which are not of the niceſt ſort: but taking a mean of various experiments it appears, that 1° of heat, according to FAHRENHEIT's thermometer, increaſes the bulk of a quantity of quickſilver of $\frac{9}{100000}$ths parts, *viz.* if the bulk of a quantity of quickſilver in the temperature of 50° is equal to 100,000 cubic inches, the bulk of the ſame quantity of quickſilver in the temperature of 51° will be equal to 100,009 cubic inches.

It is almoſt ſuperfluous to mention, that the cavity of the tubes employed for theſe experiments, muſt be perfectly uniform throughout. The ſcales to be uſed for this method ſhould be ſo exact as to be turned by the hundredth part of a grain when charged with about half an ounce weight.

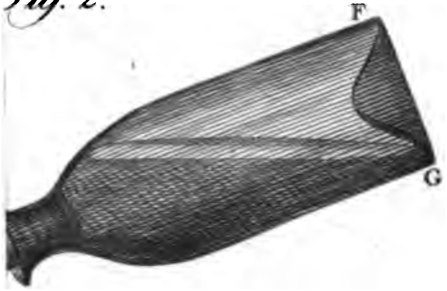
From theſe obſervations the method of graduating, or of determining the length of a degree in a new thermometer, is eaſily deduced, the only requiſites for the calculation being the weight of a quantity of quickſilver, which fills a known length of the tube, and the weight of the whole quantity of quickſilver contained in the thermometer when filled. Suppoſe, for inſtance, that in making a new thermometer it be found, that the weight of ſo much quickſilver as fills five inches length of the tube is equal to ten grains, and that the weight of the whole quantity of quickſilver contained in the thermometer weighs 300 grains. It is plain, that if the whole quantity of quickſilver weighs 300 grs. $\frac{9}{100000}$ths parts of it muſt weigh 0,027 gr. But the weight of ſo much mercury as fills five inches of the tube is equal to 10 grains; therefore, 0,027 gr. weight of quickſilver muſt fill 0,0133 inch of the tube, and this is equal to the length of 1°, or the double, treble, &c. of it is equal to two, three, &c. degrees.

By this means the ſcale may be made; that is, it may be divided into degrees, but the numbers cannot be added to them without finding which of thoſe degrees correſponds with the freezing point or boiling point. Either the point of boiling or freezing may be found by experiment, or any other point may be aſcertained by compariſon with another thermometer, and then the other degrees are nominated accordingly.

III. *Deſcription of a thermometrical barometer.*

THE determination of the various degrees of heat ſhewn by boiling water under different preſſures of the atmoſphere, has been attempted by various perſons, but it was lately completed by the accurate and numerous experiments of Sir GEORGE SHUCKBURGH, member of this Society. His valuable paper is inſerted in the LXIXth vol. of the Phil. Tranſ. Upon conſidering this paper, I thought it poſſible to conſtruct a thermometer with proper apparatus, which, by means of boiling water, might indicate the various gravity of the atmoſphere, *viz.* the height of the barometer. This thermometer, together with the ſuitable apparatus, might, I thought, be packed into a ſmall and very portable box, and I even flattered myſelf, that with ſuch an inſtrument the heights of mountains, &c. might perhaps be determined with greater facility than with the common portable barometer. My expectations are far from having been diſappointed, and although the inſtrument which I have hitherto conſtructed has various defects, I have, however, thought of ſome expedients which will undoubtedly render it much more perfect; I ſhall then preſent to this Society a more particular account of it, and alſo of the experiments which I intend to make with it. The inſtrument in its preſent ſtate conſiſts of a cylindrical tin veſſel, about two inches in diameter and five inches high, in which veſſel the water is con-

Fig. 2.



[illegible] Sc.

contained, which may be made to boil by the flame of a large wax candle. The thermometer is faſtened to the tin veſſel in ſuch a manner as that its bulb may be about one inch above the bottom. The ſcale of this thermometer, which is of braſs, exhibits on one ſide of the glaſs tube a few degrees of FAHRENHEIT's ſcale, *viz.* from 200° to 216°. On the other ſide of the tube are marked the various barometrical heights, at which the boiling water ſhews thoſe particular degrees of heat which are ſet down in Sir G. SHUCKBURGH's table. With this inſtrument the barometrical height is ſhewn within one-tenth of an inch. The degrees of this thermometer are ſomewhat longer than one-ninth of an inch, and conſequently may be ſubdivided into many parts, eſpecially if a nonius is uſed. But the greateſt imperfection of this inſtrument ariſes from the ſmallneſs of the tin veſſel, which does not admit a ſufficient quantity of water: and I find, that when a thermometer is kept in a ſmall quantity of boiling water, the quickſilver in its ſtem does not ſtand very ſteady, ſometimes riſing or falling even half a degree; but when the quantity of water is ſufficiently large, for inſtance is ten or twelve ounces, and is kept boiling in a proper veſſel, its degree of heat under the ſame preſſure of the atmoſphere is very ſettled.

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Wrecks of Ships. Inhuman practice of plundering them ſtill practiſed on the Welch Coaſt, p. 348.

Wright, Dr. William, his account of a child who had the ſmall-pox in the womb, p. 372.

ERRATA.

VOL. LXIX.

Page.	Line.	
16.	5.	*for* Goan *read* Jean
—	15.	*for* ℥iſſ. *read* ʒiſſ.
17.	1.	*for* of vitriol *read* of tartar of vitriol
18.	7.	*for* erica, formed, *read* erica-formed
—	19.	*for* wetted *read* melted
21.	13.	*for* clays *read* clay
23.	14.	*for* ℥vij. *read* ʒvij.
24.	1.	*for* ℥v. *read* ʒv.
26.	6.	*for* dale *read* deal
28.	12.	*for* ſubſtance *read* ſubſtances
30.	20.	*for* tin *read* fire
33.		laſt line, *omit the word* than
34.	23.	*after* iron *inſert* ore.

VOL. LXX.

478.	1.	Journal of the Weather at Senegambia, &c. *ſhould be* Journal of the Weather at Senegal, [For Senegambia is the whole province, the river Gambia included, in which river the weather muſt have been quite different at this period from what it was at Senegal, becauſe the diſorder did not rage there.
483.		laſt line, *read* that the wind in the ſame month in the river Gambia, &c.
490.	6.	*for* blank *read* planked

E R R A T A.

VOL. LXXI.

Page.	Line.	
334.	5.	*for* ſits *read* ſets
343.	20.	*for* flown *read* flowed
363.	3.	of the note *, *for* after a labour *read* after a natural labours.
——	9.	———— *for* eight inches *read* five inches.
383.	13.	*for* then *read* there
417.	1.	art. 23. *for* (art. 27.) *read* (art. 28.)
423.	5.	art. 28. *for* (art. 24.) *read* (art. 23.)
425.		laſt line but two, *for* $\overline{1-p}$ *read* $1-2p^2$

*** There are TWENTY-SEVEN plates in this volume.

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